

**U.S. GOVERNMENT REVIEW OF THE SECOND-ORDER DRAFT
IPCC WORKING GROUP I CONTRIBUTION TO THE
FOURTH ASSESSMENT REPORT (4AR)**

“CLIMATE CHANGE 2007: THE PHYSICAL SCIENCE BASIS”

[transmitted to the Intergovernmental Panel on Climate Change on 2 June 2006]

Review Panelists

Phil DeCola, Chair, National Aeronautics and Space Administration
Anjuli Bamzai, Department of Energy
Julio Betancourt, U.S. Geological Survey
James Butler, National Oceanic and Atmospheric Administration
Ben D’Engalo, U.S. Environmental Protection Agency
David Dokken, Climate Change Science Program Office
Randy Dole, National Oceanic and Atmospheric Administration
Kea Duckenfield, National Oceanic and Atmospheric Administration
Mark Eakin, National Oceanic and Atmospheric Administration
Dave Easterling, National Oceanic and Atmospheric Administration
Catherine Gehring, National Science Foundation
Paul Ginoux, National Oceanic and Atmospheric Administration
Peter Hildebrand, National Aeronautics and Space Administration
Martin Hoerling, National Oceanic and Atmospheric Administration
Nancy Holt, U.S. Department of State

Thomas Karl, National Oceanic and Atmospheric Administration
Yoram Kaufman, National Aeronautics and Space Administration
Chet Koblinsky, National Oceanic and Atmospheric Administration
Arun Kumar, National Oceanic and Atmospheric Administration
Eric Lindstrom, National Aeronautics and Space Administration
Matthew Menne, National Oceanic and Atmospheric Administration
John Ogren, National Oceanic and Atmospheric Administration
Michael Prather, U.S. Department of State
David Rind, National Aeronautics and Space Administration
Jason Samenow, U.S. Environmental Protection Agency
Anne-Marie Schmoltner, National Science Foundation
Dian Seidel, National Oceanic and Atmospheric Administration
Tom Spence, National Science Foundation
James Titus, U.S. Environmental Protection Agency
Robert Webb, National Oceanic and Atmospheric Administration

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
SPM	00				<p>In its current state, the SPM is a major source of dissatisfaction with this IPCC report, and the U.S. Government feels strongly that there is considerable room for improvement. It reads more like a zero-order draft than a polished effort ready for public comment. The SPM, which some might argue is the most important (and certainly most widely read) component of the IPCC report, appears to have been neglected then pieced together quickly before the Expert and Government Review. The inadequacies fall into several different categories:</p> <p>1) <i>General Approach</i>. The general approach seems to be to provide policymakers with answers to the basic questions, as defined by the respective subsections. This is not a problem per se, but policymakers have been given these answers before. What they want to know is what is new since the Third Assessment Report (TAR). While some statements are made along this line, there seems to be no organized, systematic effort to provide this perspective. It should be the first issue discussed in each of the general sections. For example: Will it really take a 3°C warming to melt the Greenland Ice Sheet? If so, that appears to be a crucial difference from what the TAR reported. One of the approaches that was well received in the TAR Synthesis Report (e.g. SPM-10a and SPM-10b on pgs. 33-34 in the synthesis report), and could be of benefit for this report, is to use graphs that combine past (proxy data), present (instrumental records), and future projections. This gives the total picture, which is somewhat missing in this SPM. The examples used in the TAR Synthesis Report are CO₂ and surface temperature. The full comparison of past and present data, with future projections should be made for other quantities as well, such as those shown in SPM-3 in the present report: sea level and snow cover.</p> <p>2) <i>Errors of Omission</i>. There are numerous aspects of the report which would be of great interest to policymakers, yet somehow have been omitted. Table SPM-1, which is summarizing some of the important elements of what is new, is somewhat confusing and it omits major new scientific understandings, with little or no justification for why particular</p>

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					<p>elements in the table have been chosen for inclusion. Somehow the loss of sea ice in the Arctic has been neglected, and benign aspects of climate change, like the longer growing season, doesn't make it in either. It is unclear what the basis is for selecting various aspects of projected change to report on, e.g., precipitation patterns are described, but information on projected changes in heavy and extreme precipitation events is not included. A list of some missing candidates follows:</p> <ul style="list-style-type: none"> • sea ice melting and tundra thawing • ecosystem movements and impact • hurricane intensity increase • sea level increase • increased rainfall intensity • increased temperatures • land temperatures • warmer highs and lows • ocean temperatures • change in storm tracks for mid-latitude cyclones. <p>Chapter 11 has been touted in the main report itself as representing the first time we are capable of providing even an estimate of regional changes; somehow this point, and everything from Chapter 11, has been omitted. Also missing are things scientists are now <i>less</i> certain of, such as the magnitude of past solar variations, a development, which is of great importance with its implications for our ability to explain not only the Little Ice Age, and perhaps a Medieval Warm Period, but the warming of the first part of the 20th century. One wonders how much thought has been put into reconciling the real achievements discussed in the overall document with what has gotten into the SPM.</p> <p>3) <i>Errors of Commission.</i> By concentrating on Greenland's contribution, this report leaves the impression that sea level during the previous interglacial was 2 to 3 m above current day values, when the chapter clearly states it was 4 to 6 m -- a very important difference. Chapter 6, the paleo chapter, recognizes that there are many uncertainties</p>

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					<p>concerning how orbital variations are connected to ice ages, yet the SPM makes it seem like a solved problem, and the associated 'radiative forcing' that is supposed to compare with that driving climate change, but is of a much different nature (latitudinal and seasonal variation, rather than global) than that of the today -- a possibly significant difference that is not mentioned. The scientific community still has little confidence in upper tropospheric water vapor trends, yet a comment is made that they are consistent with the observed warming (which is also, for this region of the atmosphere, poorly constrained). There are other examples, but it is not only that these points are incorrect, they are also inconsistent with the detailed discussion in the individual chapters, which will undoubtedly be pointed out by motivated readers.</p> <p>4) <i>Premature Conclusions.</i> There are debates or uncertainties within the chapters that accurately represent scientific debate that has not yet been resolved, but in a number of cases one particular point of view has worked its way into the SPM. For example, Chapter 3 provides a land temperature change for 1850-1910 that is very different from the SST change (which dominates the global average) given in Chapter 1. However, after 1910, the land and ocean records are very similar; do we really have good ocean temperature reconstructions for the earlier time period, globally? Before using such a controversial figure in the SPM, this should have been evaluated closely. The observations chapters say Antarctica is currently a source for sea-level rise, while the future projections have it as a sink; the SPM ignores the current assessment in favor of future modeling studies without mentioning this contradiction. The modeling studies that indicate reduced tropical storm activity in the future are very uncertain, given that models do not have the proper resolution to resolve such storms, yet somehow this very important conclusion is given credence in the report and the SPM. There is also a disconnect between Chapter 4's observational recognition that the major ice sheets are already making a net contribution to sea level, and Chapter 10's projection that they will not do so until after the year 2100. It appears that the desire to provide proof of 'progress' has led to a tendency to give conclusions in areas that are not yet decided, without the proper scrutiny.</p>

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					<p>The overall impression is that, for whatever reason, an insufficient amount of time has been devoted to the SPM to date. With additional time and attention, the U.S. Government is sure that this document will be brought up to the standards set by the previous IPCC SPM documents. A recommendation is to study TAR and SAR summaries for guidance on how to improve the current draft and incorporate some of the needed perspective.</p> <p>U.S. Government</p>
SPM	00				<p>A best estimate and a range should be given in the SPM about the residence time of CO₂ in the atmosphere. The caption to Figure SPM-2 states “No CO₂ time scale is given as its removal from the atmosphere ... cannot be expressed with a single lifetime.” This is correct as stated and appropriate for the figure caption. Elsewhere in the SPM re the multiple CO₂ lifetimes, it is suggested that the IPCC provide exact numbers and “likely” qualifiers, as appropriate.</p> <p>U.S. Government</p>
SPM	00				<p>Indicate for each appropriate item in the SPM whether the data summary represents or suggests new, different, additional, confirming, more robust data, evidence, or support for items discussed in the Third Assessment Report (TAR). The point is to make it easy for policymakers and readers to see and appreciate what new data have been obtained and evaluated since TAR publication in 2001. In each subsection, the first comments should indicate what has been learned since the TAR and to go beyond the very general, high level and sometimes lacking italicized statements currently in the document. Overall, this SPM does not really highlight the important new climate research and understanding of the last 5 years. Other than the probabilities of warming (Figure SPM-5), the policymakers could just use the TAR. Moving beyond the hockey stick and reconciling upper atmosphere and surface temperature trends both seem to be excellent candidates for the SPM authors to consider highlighting.</p> <p>U.S. Government</p>
SPM	00				<p>The SPM should clearly define uncertainties whenever they are used. This is done in the</p>

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					Technical Summary, but the SPM needs to be a standalone document, since it is often the only part of the report that is read. U.S. Government
SPM	00				All figures need to be pulled from the chapters or directly traceable to elements of chapter graphics. No new plots should be introduced into the summary documents. U.S. Government
SPM	00				All time frames need to be explicitly stated. For instance, “last 50 years” should be “since 1950” to anchor in time, tying the summary document to the TAR time frame and preventing a shifting window. U.S. Government
SPM	003	26			“experienced” is NOT the right word; try “observed in ice cores” U.S. Government
SPM	003	26			“sustained rate of increase” cannot be supported given the gaps in the ice core record and the diffusion (mixing of abundances over decade). Recommend merely deleting “sustained rate of” and retaining the word “increase”. U.S. Government
SPM	003	27			Rephrase to “over the 20th century” because we need to anchor *all* statements in time so that the document does not become dated or time periods shift before the assessment is off press. Be explicit. Do not use statements such as “past century” or “past 50 years”. Bound the time frames. U.S. Government
SPM	003	34			Insert the word “observed” after “slower than those”. U.S. Government
SPM	003	36		38	Split into two sentences. Place a period after “2004” because the two clauses are disconnected. Make the second clause a sentence. U.S. Government
SPM	003	36		39	The carbon budget is an appropriate item for the SPM, but these three questions are

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					disconnected from the underlying context and the intent of the bullet is unclear. U.S. Government
SPM	003	37		38	Change to “...global emission due to fossil fuel use, cement production, and gas flaring increase from 6.5 to 7.2 Gt C yr ⁻¹ ” to be consistent with the underlying text (page 2-3, lines 42-43). U.S. Government
SPM	003	39			“... contribute FROM 5% to 38% ...” (i.e., put units on both numbers). More importantly, this range must be defined: Is it an uncertainty? Is it just EITHER 5 or 38%? Please be explicit. Actually, the whole sentence is ambiguous. Correct it so that the reader need not interpret message on his/her own. U.S. Government
SPM	003	41		44	The statement that the sum of anthropogenic plus natural sources of methane has not been increasing although the former has doubled in value implies that the latter has declined. These are extremely weak and dangling sentences that do not say much. The second one ends with a statement that is a bit too vague for an SPM and is not entirely supportable (internally inconsistent). Suggest deletion. U.S. Government
SPM	003	42		43	Replace “growth rates” with “rates of increase”. Replace “past two decades” with actual date range in years specified. U.S. Government
SPM	004	1			In Figure SPM-1, put subtitles above the graphs to ease understanding of what is being presented without having to reference the caption. Consider a marker on the lower axis for the year 1750, as that is a critical marker year per the explanatory text. U.S. Government
SPM	004	1			In Figure SPM-1, the gray bars on the left are meant to represent the glacial interglacial range of greenhouse gases. It implies that the abundance was higher than the holocene. If so, this is BIG news and should be highlighted! If it is just an uncertainty bar, then it

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					should be dropped as it implies higher greenhouse gases in the past. U.S. Government
SPM	004	1			In Figure SPM-1, the concept of rate of change of radiative forcing is being introduced here for the first time, without any connection to climate. There is no physical connection made between dRF/dt and climate change. Moreover, the ability to take a derivative of the paleo record is very weak; this is clearly a poorly determined quantity. Either the figure shows annual records with noise (righthand side) or the authors have fit a very smooth curve through a paleo record that cannot possibly include annualized data (diffusion in the firm). This is misleading. All you show is a large derivative—which becomes a policy statement, not a scientific one. The bottom panel needs to be a simple linear sum of the top three, or deleted. U.S. Government
SPM	004	7			Replace “since” with “relative to” U.S. Government
SPM	004	9		10	Consider explaining what “low accumulation” and “high accumulation” mean, providing ranges to delineate low from high. U.S. Government
SPM	005	1			The bullets underneath and the figure do not support the header because it doesn’t connect the radiative forcing to the human influence on or composition of temperature change. U.S. Government
SPM	005	4			Replace “forcings” with “forcing increase since 1750 due to”. U.S. Government
SPM	005	5			This comment applies to 0.16 ± 0.02 , in this particular instance, but needs to be considered for all other \pm ranges as well. Define what this range means? 2/3 likelihood? 1-sigma, 2-sigma? If it has no quantitative meaning (i.e., a model range as in the Third Assessment Report), then the range should be dropped entirely. U.S. Government

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SPM	005	8			Replace “forcing” with “forcing increase since 1750” U.S. Government
SPM	005	11			In Figure SPM-2, the error bars are not associated with scientific uncertainty, but the policymaker might not realize such. The caption should be more careful and explicit as to what is contained in the figure. U.S. Government
SPM	005	11			In Figure SPM-2, consider making two columns on the right wider so that labels/titles can all be placed at the top of the columns. Consider adding, “Level of” before “Scientific Understanding” at top of last column. U.S. Government
SPM	005	13			Change “1 sigma” to “1 standard deviation” U.S. Government
SPM	005	13			Add after “global mean radiative forcings” the phrase “in 2004 relative to 1750” U.S. Government
SPM	005	13		18	Please be explicit in the text and caption about why volcanoes are omitted from the graphic. U.S. Government
SPM	005	16		17	Can a range or lower minimum number of years for CO ₂ be inserted? U.S. Government
SPM	005	21			“Aerosols” needs to be defined for the policymaker. Add a footnote defining “aerosols” at first callout (page SPM-3, line 19). U.S. Government
SPM	005	27			Replace “cloud properties” with “cloud albedo” U.S. Government
SPM	005	31			Add a bullet that covers aerosol (or total) surface forcing based on the discussion in Chapter 2 (e.g., page 7, lines 17-26 as basis). If this is done, you need to define what the surface radiative forcing means. U.S. Government

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SPM	006	6			Do not use a new measure of uncertainty. “...with a factor of 2 uncertainty...” as a way of describing uncertainty appears here without any previous explanation. In this particular case, give the range in parentheses: 0.12 Wm^{-2} (+0.12, -0.06). Readers need to be able to compare directly with the \pm ranges used throughout the SPM. Within Chapter 2 this “factor of” language is used throughout, but not within other chapters. Authors should make the method of describing uncertainty consistent across chapters <u>and</u> preface the uncertainty methodology(ies) used in the SPM. Be consistent. U.S. Government
SPM	006	9		10	Replace “radiative forcing changes” with “changes in radiative forcing”. Drop “a wide range”. U.S. Government
SPM	006	15			The idea of quoting a water vapor feedback here as 1 Wm^{-2} per $^{\circ}\text{C}$ might be valid, but there is no precedent for units like this or such a quantitative measure of feedback. Come up with a better way to express this. U.S. Government
SPM	006	15			Regarding the range “40-50%,” it is important to clearly indicate whether this range refers to transient runs (i.e., $1\% \text{ yr}^{-1}$ increase in CO_2) or to steady-state doubled CO_2 . The amplification will be different in both cases. Define carefully or delete this attribute of the estimation clause. U.S. Government
SPM	006	18			Add “Surface” before “Cooling”. Consider dropping the second and third sentences because this is dated material covered in the <u>First</u> Assessment Report. At minimum, change sentence structure to state “There remains high confidence that...” U.S. Government
SPM	006	28			The qualifiers “improvements and extensions” implies that there has been no new data, no discoveries, no different types of data! Consider rephrasing. U.S. Government

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SPM	006	31			The syntax of the sentence conveys that the temperature of “snow and ice” has warmed, rather than the extent/duration of snow/ice cover has retreated consistent with warming. Please express the results better. U.S. Government
SPM	006	32			Substitute “combined” for “joint”. U.S. Government
SPM	006	34			Be more specific in terms of year(s) regarding what is meant by “on record”—that is, “since ____” specifying a year. Explicitly state what record and the length of the record. The current form is too vague. U.S. Government
SPM	006	35			Avoid statements such as “last five years” and bound the time frame; otherwise, you risk having language that is dated before publication or at minimum a confusing window. Give the exact time period. U.S. Government
SPM	006	35		36	Bracketed language indicates that data will be updated. Is the intention to carry this tack throughout the SPM themes/bullets as appropriate in order to provide the policymaker with the most current data before going into production? U.S. Government
SPM	006	38		42	The use of different temperature changes over different periods is confusing. It is difficult to compare 0.6 ± 0.2 with 0.65 ± 0.2 for 5 additional years. Are they significantly different given the uncertainties? Also, define what the range means. The current text states that temperature has increased since 1850, but only gives a number from 1900. U.S. Government
SPM	006	39			Be precise in the time spans referenced. Be consistent with the time rate of change for temperature. U.S. Government
SPM	006	41			The large trend in warming that was observed in the 1910-1945 period needs to be

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					explained relative to the more recent trend. If the former was natural variability, why not the latter, which is of similar magnitude? This distinction is particularly important for policymakers. This discussion might be more suitable for the Attribution section of the SPM (page SPM-10), but warrants mention here. Can we say <u>why</u> “most of the warming” occurred during the referenced periods vs. other periods in the 20th century? Or what the cooling factors might have been? Would it be desirable to indicate/reference why the rate of increase has been larger for the 1979-2005 period? U.S. Government
SPM	006	41		42	This is a significant change from the TAR. Please elaborate and add uncertainty qualifiers to the urban heat island discussion. Why can we discount the urban heat island effect? U.S. Government
SPM	006	42			Replace “large-scale values” with “results”. The hyphenated modifier is jargon. U.S. Government
SPM	006	44		46	Should remaining discrepancies in the tropics (per CCSP S&A Product 1.1) be mentioned or at least referred to (as not everything is perfectly reconciled)? A possible way of handling this would be adding “in most areas” after “...that are consistent with the surface temperature record within their respective uncertainties” or adding “mostly” before consistent. U.S. Government
SPM	006	51			“Global average water vapor” is a single value. It does not have land and ocean and upper and lower troposphere values. Consider rephrasing to state “On average, the atmospheric water vapor content is increasing...” In addition to global increases, there are changes over land and oceans. U.S. Government
SPM	006	51		52	This statement is not consistent with the Chapter 9 Executive Summary. Break this apart so that you have a period after the word “ocean.” As it’s own bullet, discuss the upper troposphere, including the caveats given in the chapter. Remove “in a manner consistent

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					with warming” which is an attribution statement and inappropriate for the Observations subsection. U.S. Government
SPM	006	52			Consider adding explanatory language, if appropriate, like “because of increased rates of evaporation caused by observed warming.” Can you say anything about changes in evaporation rates? U.S. Government
SPM	007	1			Do you mean the global average temperature to a depth of 3,000 meters? Can you put real trends on it? U.S. Government
SPM	007	1			For ocean temperature, avoid the term “risen” because it sounds like a sea level qualifier. Substitute “increased”. U.S. Government
SPM	007	2			Does 2 Wm^{-2} apply to the “entire Earth’s surface” or just over the ocean fraction? Be explicit. U.S. Government
SPM	007	7			Be more specific with regard to what the “changes in heat content” have been. Regarding time frame, why start in 1961? U.S. Government
SPM	007	10			The labels on the vertical axis are confusing. The top plot should be something like “Difference from 1961-1990 Mean (°C)”. Middle plot should be consistent with this: “Difference from XXXX-YYYY Mean (mm)” (caption does not say what reference period is used to calculate sea-level anomalies). Normally the unit of sea-level rise should be height/time, but present plot shows anomalies in mm. Caption should be changed accordingly. U.S. Government
SPM	007	10			There appear to be inconsistencies between the global land surface temperature trends in

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					Figure SPM-3 panel (a) and in Chapter 3; specifically, global mean temperature is in disagreement with that depicted in Figure 3.2.1, which shows the global land temperature for the period 1850-1919. One might argue that it is the difference between global and land temperature trends, but after 1910 they are in good agreement. Perhaps the land temperature record was too sparse prior to 1910 (but how extensive was the ocean temperature record for that period)? This disagreement has implication for the temperature trends since it is the mean value for this earlier period that is being compared against in the SPM (the average in Figure 3.2.1 appears considerably lower). Is this chosen for effect? Cite exactly which figures were distilled to construct the SPM figures so that readers don't have to guess. U.S. Government
SPM	007	10			In Figure SPM-3 panel (a), the uncertainty depicted is inconsistent with the intention of the figure—which is, presumably, to show overall temperature changes since 1850. The uncertainty shading must be added to the structural uncertainty of the differences among the three (at least three) global data sets, including the NOAA and NASA data sets. The differences between all three data sets represent an estimate of the structural uncertainty in using various approaches to adjust for data biases. The shaded band, although not stated, only represents the parametric uncertainty for one data set. To fix this, plot a band bounded by all three global data sets. In Figure SPM-3 panel (c), some estimate of decadal uncertainty should be provided or indicated in the legend, if no estimates are available. Construct needs to be reevaluated to reflect multi-decadal uncertainty in the trends, particularly because two data sets are being used. U.S. Government
SPM	007	10			The figure caption talks about the change from the first 70 years of instrumental record (1850-1919). Make the figure correspond to the caption (which is done well) by making the zero delta-T for this 70-yr average. U.S. Government

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SPM	007	12	8	2	This is a new period for calculating the trend (1850-2005). This is different from the Third Assessment Report. Why? Give a rationale why the first 70 years are used as a baseline (why 1850-1919?). Make sure that all figures in this conglomerate are traceable. Where do the uncertainty bars come from? U.S. Government
SPM	007	17			Add the words in all caps to the existing caption "...MEAN sea level, and NORTHERN snow cover area IN APRIL." U.S. Government
SPM	007	17			The sea-level rise is deceptive/misleading here, since the implication is that the entire rise is due to warming. Adjust the caption to make more precise. U.S. Government
SPM	008	3			"land ice" - what is this? is it snow, glaciers only? ALSO, drop the parenthetical (particularly those that ...precip) it may be a random factoid here, but it is really is confusing here and does not add to the understanding. U.S. Government
SPM	008	9		10	Why is April the only month mentioned? Recommend including a generalization of trends at other times of the year lest there be accusations of "cherry-picking." U.S. Government
SPM	008	14		15	All the material under this header does not reflect a finding in Chapter 4 that the Antarctic and Greenland ice sheets are shrinking. Why is that not repeated here? Also the last two bullets seem to fit better under an Extremes heading than Systematic Change. U.S. Government
SPM	008	17			Explain "Widespread increases in warm..." What is this? An increase in the warmth, the extreme, the extent? Please rewrite in plain terms. U.S. Government
SPM	008	21			What sorts of "Changes"...in mid-latitude westerly winds, etc. U.S. Government

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SPM	008	29			What is not noted here that is very significant is that the 1940s showed a very different latitudinal pattern from that of the 1990s, and what might be expected from greenhouse gases. This is a key result when comparing these two very different warm periods and is missed entirely by this summary. U.S. Government
SPM	008	29		35	Move the last sentence of the third bullet to its own bullet. Remove the reference to Antarctic in line 34. Greenland and the Antarctic should be their own bullets. Then clarify message. The implication in the current text is that there are no trends when in reality there are (e.g., the warming of the Antarctic peninsula). U.S. Government
SPM	008	30			Rework sentence for clarity. U.S. Government
SPM	008	36			Change “in Antarctica” to “for Antarctica” U.S. Government
SPM	008	39		40	It is stated that droughts have increased but there is no indication about locations of the increase, duration, frequency, or magnitude. This assertion needs to be substantiated with additional detail. U.S. Government
SPM	008	39		42	This statement is not supported by the observations in Chapter 3. Overall precipitation has increased since 1900, with only recent decreases (last few decades) in the tropics. At best the statement requires temporal and spatial qualification, but more generally it seems biased not to mention that droughts may have decreased where precipitation has increased (high latitudes). There is concern about the statistical significance of any trend in droughts. Of special concern is whether this statement is based on the diagram related to the drought question in Chapter 3. There the first PC is shown with an increasing trend toward droughts, but it reflects only 6% of the total variance. A better statistic would be the percentage of land areas affected by moderate to extreme drought. The drought bullet

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					should be dropped in its present form. It does not contribute much and is not of enough significance to be included in the SPM. U.S. Government
SPM	008	43		46	Restate what the “trends” are that are referenced in this bullet. U.S. Government
SPM	008	49			Rephrase to “Increases in heavy precipitation events ARE observed, even in....” U.S. Government
SPM	008	54		55	Are there really concerns about the tropical cyclone data from the satellite era? The numbers of storms is certainly unquestioned. Landfall data are a poor statistical sampling. Explain exactly what data set quality is of concern, because a blanket statement as currently phrased condemns all recent observations. U.S. Government
SPM	008	55			Please be specific with the dates. U.S. Government
SPM	009	1			Table SPM-1 seems unbalanced in terms of suggesting negative outcomes. For example, decreases in cool nights and frosts might be accompanied by “longer growing season” just to add some balance. The statement on increase in summer mid-latitude droughts seems to rely too heavily on PDSI estimates, and even here the trends are not universal since the United States and China do not show strong evidence of increases. Moreover, some references in Section 3.3 (e.g., Robuck, 2000; Hirabayashi <i>et al.</i> , 2005) do not suggest negative trends in soil moisture. U.S. Government
SPM	009	1			Table SPM-1 has serious problems. In the previous summaries (e.g., 2001 SYR Table SPM-1), there were at least 25 phenomena and now just 8. How did the authors pare back to this number? Perhaps the writing team should consider updating and augmenting the last table as a starting point. With regard to specific fixes for this rendition, either include quantification or explain

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					<p>why not.</p> <p>Eliminate confusion about how the phenomena were chosen. State clearly what the objective criteria are for inclusion or exclusion in this table. In the table legend, link the “Phenomenon” column header with “Projected Trends for the 21st Century”. Is the rationale that the contents in the table are items that have high confidence? This would allow labels of moderate to high confidence, thereby excluding lower confidence events? Confidence based on statistical correlation of trends above the noise of natural variability? A basis for the selections eliminates contentions of bias. There are obvious things that are missing (growing season, Arctic sea ice, temperature, water vapor, sea level, ...). Note that the absence of trend or confidence in trend is valuable information for policymakers. In sum, the authors need to include a very clear statement about why these particular items are here.</p> <p>U.S. Government</p>
SPM	009	1			<p>In reference to “Warm days/warm nights increase over mid-and high-latitude land areas,” does this language suggest no increase over low-latitude land areas? Remove the parenthetical in the “Droughts” entry. There is no evidence provided in Chapter 3 that droughts have increased in intensity in mid-latitude summer, other than some inference that when it gets warmer in summer there is generally less precipitation. At minimum, the “increase” in droughts needs to be explained in terms of extent or duration. The third column entry for the drought row should be in italics.</p> <p>U.S. Government</p>
SPM	009	1			<p>Telling a policymaker that something is more likely to have happened, but with only low confidence—which is defined as about 2 out of 10 chances of being correct—is a highly mixed message. Given the low level of confidence, the correct assessment is that it is unknown whether the increase in tropical cyclone intensity can be attributed to human activities. Change the evaluation of human influence on mid- to high-latitude cyclones to “unknown.”</p> <p>U.S. Government</p>

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
SPM	009	1			Table SPM-1 is awkward and not well balanced: (1) It really needs to have temperature and water vapor listed first (and included), including temperature patterns as these are the first of the attribution studies and still the most important; and (2) the subtle difference between a “formal” attribution study (roman) and “other” (italics) is quite vague, since similar conclusions on likelihood appear for both. The header “Confidence in Trend Predicted for 21st Century” is not helpful, since there could be a reverse trend. Perhaps rephrase as “Confidence that the 20th Century Trend will Continue” or else explain what the 21st century projections are. Italics stand out, so why use it for the marginal cases? U.S. Government
SPM	009	1			With regard to the bottom two entries in Table SPM-1, “Not assessed” seems arbitrary since undoubtedly some research has been performed on the subject. Perhaps “Not assessed by this report” is more accurate. Also what constitutes a high sea-level event? Would “storm surge” be more apt? U.S. Government
SPM	009	7		9	Box TS 1.1. defines “High confidence” as about 8 out of 10 chances of being correct, and “medium confidence” as about 5 out of 10 chances of being correct. Table SPM-1 introduces a different definition of high confidence and a new term: moderate confidence. The treatment of uncertainty in this report is difficult enough to follow without changing terminology. The terminology defined in Box TS 1.1 should be used. Amplification of the definitions—i.e., “It is our judgment that this statement has 8 out of 10 chances of being correct because ...”—would be useful. U.S. Government
SPM	009	15			Replace “thousands” with “hundreds” U.S. Government
SPM	009	15		17	Replace sentence with: “Individual proxy measurements (e.g., tree-ring width) can be influenced by single (e.g., temperature) or multiple environmental factors (e.g., both temperature and precipitation) operating during one or more seasons, in some cases integrated over the temporal resolution of the particular record.” The authors are encouraged

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					to try to convey this information in a shorter distilled form. U.S. Government
SPM	009	22			The phrase “a larger warming since the early 19th century” seems a non sequitor. Delete the whole sentence. U.S. Government
SPM	009	26		28	Add after “125,000 years ago” the phrase “it is virtually certain that sea level was 4-6 meters higher than present and”. Replace “to a sea-level rise above current levels” with “of that total.” Add a sentence: “Paleoclimate observations also suggest that the Antarctic ice sheet likely also contributed to the last Interglacial sea level increase.” (from page 6.2, line 55 to page 6.3, line 1) U.S. Government
SPM	009	28	10	1	“This is associated with” and “linked to” are vague verbs. Also, reference to “forcing due to changes in the Earth’s orbit around the Sun” is ambiguous. Are these positive or negative forcings? Rewrite sentence as follows: “This melting can be explained by model simulated Arctic summer temperatures about...” U.S. Government
SPM	010	8		12	The second sentence of the italicized paragraph says that there’s an increase in confidence since the TAR, when in line 14-15 the very same confidence level as in the TAR has been used. In lines 9-10, does “stronger signals emerging in longer records” merely mean that the last 5 years have extended the series of warmest years on record? This is unclear to the reader. U.S. Government
SPM	010	12			Add “in the simulations” after “although uncertainties remain” U.S. Government
SPM	010	14			Add some discussion—as a new first bullet—of the likelihood of anthropogenic forcing as related to the warming of the first half of the 20th century, as shown in the figures. Attribution for at least the whole 20th century needs to be addressed, including periods of

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					warming and level periods. U.S. Government
SPM	010	14	11	33	Comparing these two statements may confuse readers: (1) on SPM-10, line 14, “It is very likely that greenhouse gas forcing has been the dominant cause of the observed warming of globally averaged temperatures in the last 50 years.”; and (2) on SPM-11, line 33, “Attribution studies considering the entire record of the past 700 years support the conclusion that it is likely that greenhouse gas forcing has been the dominant cause of the observed warming of the northern hemisphere over the last 50 years.” Why is one statement “very likely” and the other only “likely”? It takes a careful reader to see that the “very likely” refers to the globe and the “likely” refers to a hemisphere rather than the 700-yr record. Double-check that you mean the global temperature change due to humans is very likely, while the northern hemisphere change due to humans is only likely and the southern hemisphere change is uncertain. U.S. Government
SPM	010	15			The phrase “last 50 years” should be “since 1950” U.S. Government
SPM	010	17			The first sentence should really be last, it would be much better to begin with the strong statement “Anthropogenic warming...” and finish with the qualifiers. U.S. Government
SPM	010	25		26	Clarify “observed” (over what time period or since what year)? Similarly, what is meant by “recent”? U.S. Government
SPM	010	25		27	The conclusion here applies either to “natural forcing and internal variability” or this <5% likelihood applies only to internal variability. If the latter, this conclusion is very misleading and the word “natural” should be dropped, as most will read this as all “natural” forces (vs. anthropogenic). If it is only internal variability, then this is not

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					important enough for an SPM bullet. No matter what construct remains (if any), if retained in the text, authors need to clarify what it means by natural INTERNAL variability and natural EXTERNAL variability. U.S. Government
SPM	011	1		2	“The signal of greenhouse-gas forcing” phrase needs to explicitly include GHGs + aerosols. In line 2, add the all caps item to the existing sentence “...temperature CHANGES.” U.S. Government
SPM	011	3			“very small” is not a standard term. Edit to use a standard term. U.S. Government
SPM	011	6			Delete “predictable” and replace “due” with “attributable” U.S. Government
SPM	011	8			The word “other” stands out. It suggests reference to preceding bolded entry on SPM-10, lines 14-15. Delete “other aspects of climate including”. U.S. Government
SPM	011	11			“Anthropogenic forcing” is a vague term and is mixed here with “human influence”. Avoid using “anthropogenic forcing” unless it is explicitly and carefully defined in the SPM. Also include definition in the glossary. U.S. Government
SPM	011	15			“Human influence” also needs definition in the glossary. U.S. Government
SPM	011	15		18	This bullet is misleading about the attribution of human influences on changes in precipitation patterns. Replace “related variables” with “related storm tracks”. Also in line 9, remove “and precipitation” because circulation effects on precipitation are not sufficient to determine changes in total precipitation. U.S. Government
SPM	011	17		18	This last “however” sentence does not enhance understanding of the first or serve as a

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					suitable caveat. Change to “Differences between model simulations and observed changes are...” Given this new more accurate statement is it strong enough to be included in the SPM? U.S. Government
SPM	011	20			Add the word “fewer” before “frost”. What is the time frame of the trend? This comment rendered moot if time frames elucidated in Table SPM-1. U.S. Government
SPM	011	21		22	The IPCC lexicon of likelihood has not been applied in this statement about possible attribution of the increase in heat waves to human-induced warming. Rather than saying human-induced warming “may” have increased the risk of heat waves, shouldn’t it say it is “likely” to have increased the risk of heat waves or something similar? The word “may” does not provide any information regarding likelihood. In addition, change “risk” to “frequency”. U.S. Government
SPM	011	27			Quantify “a large fraction” or merely change to “some”. Go back to the chapter and make consistent. U.S. Government
SPM	011	29			Change “causing” to “which cause” U.S. Government
SPM	011	33			Another forcing qualifier is now introduced: “greenhouse gas forcing”. Two similar terms have been used prior (e.g. “anthropogenic forcing”). Limit the use of terms or be sure to clearly define the differences. Greenhouse gas forcing is the correct term but need to clarify in this particular instance by “anthropogenic forcing from greenhouse gases”. U.S. Government
SPM	11	34		35	Capitalize proper nouns Northern Hemisphere and Southern Hemisphere U.S. Government
SPM	011	40		52	This is too much detail and not really useful in the SPM, and provides opportunities for

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					internal inconsistencies. Emphasize briefly what is new since the TAR (for this and all other preambles). U.S. Government
SPM	011	40			Delete “fully”. Also consider deleting “coupled”. The first is suspect, and the second does not really convey anything to a policymaker. U.S. Government
SPM	011	45			The phrase “possible future conditions” is problematic. The word “projected” is generally used. And what is meant by “condition”? Do the authors mean atmospheric composition? U.S. Government
SPM	011	47			The use of the term “commitment” is a serious mistake in the SPM. The idea that freezing greenhouse gases at today’s levels is a “commitment” to future warming is a decades-old concept, originally (and still should be) called “unrealized warming” (Hansen). More importantly, the term “committed warming” has a standard dictionary definition, and should not be used for a specific mind-experiment that will not happen. Governments are committed to much more warming, since the increase in CO ₂ cannot be stopped cold—as shown by the IPCC TAR stabilization scenarios, which have CO ₂ rise before a reduction in fossil carbon dependency and an eventual leveling off at values well above those today. These are the true “commitment” studies. The SPM authors should not misuse the term to describe a purely gedanken experiment. U.S. Government
SPM	011	48			There is reference to “SRES marker scenarios”. Provide a footnote that steers readers to this IPCC Special Report (i.e., a complete cite). U.S. Government
SPM	011	49		50	Does this really provide new info on the physical aspects of climate change that we did not know before? U.S. Government
SPM	011	51			“plausibility or likelihood” pick one or the other, not both.

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
SPM	012	01			The improvement also involves the ability to marry ensembles. That is as important as model improvements. Also define “near-term”. U.S. Government
SPM	012	05			The question is “stabilized WHEN and at WHAT levels”? CO ₂ cannot be stabilized at current levels. This is an obvious conclusion and should be dropped. U.S. Government
SPM	012	10		11	Confirmed projections made in earlier IPCC reports cannot be used to argue for greater confidence in today’s assertions. We have no short-term prediction skill. Further, what is “short-term”? U.S. Government
SPM	012	12			This sentence should add “...assessments, and the limited ability to change the trajectory of greenhouse gas increases.” Or something to that effect. Clarification needed. U.S. Government
SPM	012	14			The stabilization scenarios have a trajectory. What is meant here is “fixed instantly”, not “stabilized”. U.S. Government
SPM	012	21			Clarify “Further”. U.S. Government
SPM	012	24			The left panel of Figure SPM-5 needs some work. What does the horizontal bar with the circle mean? Put a vertical line at the zero (1980-1999) mark. It would be very good to add a vertical dashed line showing the 1850-1919 value. Label both of these lines so that the X-axis label is then clear (i.e., relative-to-what). Expand the caption to fully explain the lefthand panel. U.S. Government
SPM	013	1			Is not 1-sigma 68%? U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
SPM	013	3			Delete “are scenario dependent” since obvious and unnecessary. U.S. Government
SPM	013	7		8	The language describing the most “likely” climate sensitivity is confusing. Since likely suggests a 66-90% probability and climate sensitivity is usually described by a PDF, it’s not clear how the so-called “most likely” climate sensitivity was determined. Does this reflect multiple PDFs? U.S. Government
SPM	013	7			Be explicit on climate sensitivity. Use “climate sensitivity to doubled CO ₂ ” everywhere where that is the intent to be expressed. The shorthand jargon can have multiple meanings. U.S. Government
SPM	013	09			The “Values substantially...” sentence should be recast as “Models with higher than 4.5°C are unlikely(?) in terms of representing current climate....” Do not exclude anything. U.S. Government
SPM	013	12			“...warming for a range of scenarios shows geog...” is not truly scenario-independent. Consider massive volcanoes. U.S. Government
SPM	013	21			Define “upper layer”. Is this 1 cm or 100 m? U.S. Government
SPM	013	24			This is a very important bullet. It should be the first in the list. Does the literature address the issue of longer lived; and if so, that may be the most important finding. The last sentence needs to have the final clauses reversed or rewritten. U.S. Government
SPM	013	26		27	The sentence is very awkward. Rework. U.S. Government
SPM	013	33		35	Suggest editing sentence to read (underlined text added): “These changes are linked to <u>projections</u> for fewer but more intense storms...” to make it clear the link references a projection as opposed to an observed change.

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
SPM	013	38			Intent not clear. “In general, decreases in the number/area of regions becoming dry, and increase in those becoming wetter??” Dry and wet are associated with soil moisture, not precipitation. U.S. Government
SPM	013	41		44	This bullet is confusing. The increase in pH occurs 'over' the 21st century, not sometime 'in' the middle of... Add “many” before “marine”. Is the 0.1 decrease since pre-industrial times an observation or just a model calculation? “marine calcifying organisms” seems too technical for the SPM. At minimum, add to glossary. U.S. Government
SPM	014	22			Do not see how process knowledge has much to do with “policy options.” It does not apply to all the sub-bullets here. The processes do not necessarily lead to better understanding of climate stabilization either. The simple statement that “Stabilization of radiative forcing is a prerequisite for climate stabilization” should be a separate bullet, perhaps immediately after line 23. U.S. Government
SPM	014	22		23	This section should include mention of the level of certainty of expected climate change on regional scales relative to global scales. U.S. Government
SPM	014	25			First sentence is wrong. The major issue is the projection of greenhouse gases (more important than climate feedbacks). U.S. Government
SPM	014	25		27	“Very long-term...”? How long is that? “more uncertainty” than what? Delete “of slow feedbacks” because it is not correct. Fast feedbacks are also not well observed or understood. “processes” alone likely captures intent. Inertia needs a new section or bullets, and should not to be tacked onto this bullet as a throwaway. There are too many different points being made, some of which even conflict (e.g., the observations sentence and the

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					stabilization one). U.S. Government
SPM	015	4			How likely is this added 1.2°C? Uncertainty qualifiers must accompany numbers like this in the SPM. U.S. Government
SPM	015	5		7	Difficult to discern that “Alternatively” did not refer to an alternative to the positive feedback effect but instead to an alternative emission scenario (stabilization instead of SRES). Since carbon dioxide stabilization is not mentioned elsewhere in the SPM, why not delete this sentence? Or there may be a simpler statement possible: “The presence of positive feedbacks in the carbon cycle means that further increases in CO ₂ emissions will lead to more than proportional increases in atmospheric concentrations.” U.S. Government
SPM	015	5			What does “it” reference, the “feedback effect”? U.S. Government
SPM	015	9			Important to note the other greenhouse gases, but this bullet is fairly weak. Strengthen the bullet and clearly connect to air quality. U.S. Government
SPM	015	9		11	Include “in addition to carbon dioxide” after “aerosols” U.S. Government
SPM	015	13		14	There’s a need to explain what the implications are of the Atlantic meridional overturning circulation (MOC) by up to 60% by 2100. Consider linking to abrupt climate change. U.S. Government
SPM	015	19			Not all sea-level rise has much longer times scales. The authors need to note the difference between melting and warming (look at the TAR Synthesis Report’s treatment of inertia). Rewrite this bullet to make clear the different processes and time scales (i.e., short-term rise in sea level). U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
SPM	015	25		28	The reader may also think of large floating ice sheets: “Changes in the Antarctic and Greenland grounded ice sheets MAY signific.....” Also, has the number changed from the TAR? It used to be that a warming of 3°C AT GREENLAND would melt it, attainable by a 1.5°C global mean warming (not 3°C as noted here). If correct as written, note the change since the TAR because this is significant. U.S. Government
SPM	015	25		32	This bullet needs be reworked in its entirety. For example, “This level of warming” is an awkward sentence construct. Does “this” is refer back to up to 0.4 per century? There is a jump at the end to paleoclimate data suggesting a sea-level rise contribution of several meters. U.S. Government
SPM	015	29			Replace “occur” with “be reached” U.S. Government
SPM	015	31			Change to read “...a comparable response to warming during the last ...” U.S. Government
SPM	015	32			Replace “widespread Arctic” with “polar”. Change “several” to “4-6”. U.S. Government
SPM	015	34		39	Change “It is expected to gain ice through increased snowfall in the 21st century, acting to reduce global sea-level rise by about 0.1 m per century” to “Models suggest that it will gain ice through increased snowfall in the 21st century, acting to reduce sea level; [10.7] but observations suggest that it has lost ice and added to sea level during the last decade [4.6].” Chapters 4 and 10 reflect different points of view; the SPM ought to reflect both points of view. U.S. Government
SPM	015	38			Replace “could” with “might” U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
TS	00				Most paragraphs in this chapter are headed by bullet-like points in italics. Some of these use the phrase “...with a...level of scientific understanding.” It would be good if use of this indication were done more consistently. U.S. Government
TS	004	38		39	The definition for “More likely than not” (i.e., more than 50% likelihood) is too nebulous a definition to be useful. Since “likely” is defined as >66% probability, the implication is that “more likely than not” covers the range 50-66%. If this is the case, it should be clearly stated. If not, some additional information should be provided as to what “more likely than not” means. U.S. Government
TS	004	38		39	The definition of “More likely than not” (i.e., more than 50% likelihood) is vague, since it gives no indication of an upper bound. WG I needs to either provide an upper bound for the term “more likely than not” or not use it. U.S. Government
TS	006	44			Change line to: averaged 0.2 ppb yr ⁻¹ (0.01% per yr) for the 6-year period from 1999 to 2005. If the value is really so sensitive to the inclusion of 2005, then some mention of this sensitivity is appropriate. Otherwise, it looks like the report is cherry-picking. U.S. Government
TS	007	3			Change to: “...global emission due to fossil fuel use, cement production and gas flaring increase from 6.5 to 7.2 GtC/yr” to be consistent with the underlying text (Pg. 2-3, lines 42-43). The underlying chapter (Pg. 2-3, lines 42-43) indicates that the emissions cited here are from fossil fuel use, cement production, and gas flaring. Change the text to indicate that these emissions are from all three sources, not just fossil fuel use. U.S. Government
TS	007	8		11	While gas flaring is technically a fossil fuel use, Table TS-1 should be changed to indicate that the emissions are from fossil fuel use, cement production and gas flaring to avoid any misinterpretation and to be consistent with the underlying chapter.

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
TS	007	21		22	The statement “...CO ₂ uptake by the ocean is not linked to a corresponding O ₂ flux” confuses me. This statement seems appropriate for CO ₂ going into solution in the ocean, but some of the flux of CO ₂ into the oceans will be associated with photosynthesis, and so will have a compensating O ₂ flux. A quick look at section 5.4, referenced here, seems to support my understanding, but I remain somewhat confused. U.S. Government
TS	008	34		35	Is it remarkably large or small interannual variability? U.S. Government
TS	009	26			“...factor of three uncertainty...” as a way of describing uncertainty appears here without any previous explanation. Within Chapter 2 this language is used throughout but not within other chapters as much. May want to make the method of describing uncertainty consistent across chapters or at least contain a prefacing discussion in the TS. U.S. Government
TS	010	7		8	“...now considerably better understood...” and “...low level of understanding” seem contradictory. How would you characterize the level of understanding at the time of TAR? U.S. Government
TS	010	7		8	How can direct aerosol radiative forcing be described as considerably better understood than in the TAR if it is still given a low level of scientific understanding (the same as it had in TAR)? U.S. Government
TS	011	3			“...factor of two uncertainty...” as a way of describing uncertainty appears here without any previous explanation. Within Chapter 2 this language is used throughout but not within other chapters as much. May want to make the method of describing uncertainty consistent across chapters or at least contain a prefacing discussion in the TS. U.S. Government
TS	012	2			There’s no comment about how volcanic activity in the last 50 years has changed relative

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					to previous (even pre-industrial) time periods; at least say it is uncertain. U.S. Government
TS	012	11		13	Either provide a basis for this statement or delete it. The statement does not appear in the Executive Summary, Synthesis section, or in the underlying text of Chapter 2, and is not intuitively obvious from the material presented in the Technical Summary U.S. Government
TS	012	16			Section TS.2.5 should contain mention of the Annual Greenhouse Gas Index developed by the NOAA/ESRL Global Monitoring Division (formerly Climate Monitoring and Diagnostics Lab, see http://www.cmdl.noaa.gov/aggi/). This sums the radiative forcing due to a variety of long-lived, well-mixed greenhouse gases, and normalizes the total to 1990 values. It has certain limitations and caveats; these should be placed in the main text and cross-referenced in the TS. U.S. Government
TS	012	36			The text explicitly states that GCM stands for “global climate model” while the glossary says “general circulation model.” Pick one. U.S. Government
TS	012	042			Very misleading statement. Orbital variations during the HOLOCENE represent a completely different type of climate forcing, not at all indicative of the ability of models to assess future climate response. Many questions remain about the ability of models to depict the LGM world, or indeed what that climate was really like in the tropics. Large uncertainty exists in how orbital variations, particularly the 100K cycle, could drive ice ages. All of these effects are underplayed in this paragraph. U.S. Government
TS	013	3		45	An estimate of the radiative forcing associated with the Milankovitch cycle would put this process in the same context as the radiative forcing associated with anthropogenic processes. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
TS	018	1	33	22	The structure of first describing the individual components of the climate, atmosphere, ocean, cryosphere, then the consistency between the components leads to an unnecessarily long and difficult to follow description of Observations of Changes in Climate. What matters to climate is the interaction between the three components (i.e., the consistency between the components). Recommend that the pertinent facts from the individual component descriptions be integrated in the consistency discussion. For example, under the heading “Changes in the atmosphere, cryosphere and ocean strongly support the view that the world is warming,” place the information from the three previous sections that support this statement. This would give the reader the pertinent information in one location and obviate the need for continuous referral to the previous sections to find the evidence for this statement. Using this approach would also place the observations into an understandable framework rather than just a list of findings without context. U.S. Government
TS	018	42			Please elaborate on the phrase “not a good fit to the data” and discuss the warming during the period 1910 to 1945 as recommended in comments on the SPM and Chapter 3. U.S. Government
TS	018	53			DTR to be spelled out and in a table of acronyms? It may also be more sensible to move TS-19, lines 1-3, before this point so that the idea of changes in DTR does not come out of the blue. U.S. Government
TS	018	54		55	This sentence has some crazy syntax. Also why does it contrast urban heat islands to oceanic warming, rather than the more general global-scale warming? U.S. Government
TS	019	40			However, the more positive west winds have not continued in the 2000s, which sort of invalidates this discussion. U.S. Government
TS	020	20		50	Some of the information in this box is old and not consistent with discussions later on in

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					the text. For example, in the ocean section, it is frequently stated that the frequency of ENSO events has changed, thus invalidating the preferred time scales for these events given in the table. Similarly, the fact that the NAO may be a component of the NAM as stated in other section should be made in this box. U.S. Government
TS	020	37		39	Text mentions correlation between NAM and NAO, but not between PNA and ENSO. U.S. Government
TS	021	20		21	A sentence or two on how correlations between SST and water vapor support a 4% increase in the latter is needed. U.S. Government
TS	021	27		32	The International Satellite Cloud Climatology Project shows a decrease in cloud amount in the tropics from 1985. The paragraph is very confusing and needs to be rewritten. Is the decrease in DTR related to a change in the diurnal cloud amount? U.S. Government
TS	021	41		45	Consider separating ideas into more sentences. U.S. Government
TS	022	21		22	Variations in the number of tropical cyclones have been shown to be dominated by multidecadal variability and not decadal variability (e.g., Goldenberg et al.) U.S. Government
TS	022	26		27	Please clarify the phrase “there are concerns about the quality of the historical data” since the phrase can apply to both the pre-satellite and satellite era. There are reasonable concerns about changes in the satellite data used to classify hurricane strength. U.S. Government
TS	022	29			What is the basis for the claim that there is a trend towards longer lifetime and greater storm intensity? Figure TS-13 supports a finding that sea surface temperature has been rising, but no more. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
TS	022	34		36	There should be some quantitative evidence here to substantiate changes in drought frequency, duration and/or magnitude. Also, information about location of changes in drought characteristics should be mentioned. U.S. Government
TS	023	4		10	Aren't satellite observations of snow cover for the SH available for the same period as for the NH (i.e., 1966-2004)? If so, why can't similar trends be determined for the SH? U.S. Government
TS	023	25			Should read: "Temperature at the top of the permafrost has increased" U.S. Government
TS	023	25		26	Drop this sentence since bottom up thawing is due in many places to loss geothermal gradient and not present climate. The 0.4m does not agree with the 0.01-0.02 in text (Chap 4; page 4-31). U.S. Government
TS	023	46		48	Minority portions of above-hydroface ice melting may contribute to sea level rise. U.S. Government
TS	023	56		57	The observation of widespread glacial and ice cap mass retreats during the 1930s and 1940s and the relation of this retreat to anthropogenic versus natural causes needs to be explained here. U.S. Government
TS	024	2			Sea level equivalent needs to be defined. U.S. Government
TS	24	22		31	The presentation in this paragraph is incomplete. The underlying chapter (Pg 4-27, lines 8-9) includes the assessment that "Large ice-flow models do not accurately capture the physical processes involved in such dramatic iceberg calving (as the breakup of Larsen B), or the more common calving behavior." This assessment indicates that we lack the tools for forecasting the contribution of ice flow to sea level rise. This point is made less clearly in Box TS 3.2, but it should be related directly to the behavior of Larsen B, as was done in

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					the underlying chapter. U.S. Government
TS	024	27	25	14	Within ice sheet, above basil interface, is density stratification and interstitial liquefaction included in dynamics/stability prognosis? U.S. Government
TS	025	1		4	The first two sentences provide different details on ice sheet shrinking and growing. Specifically, the first sentence states that shrinking occurs faster than growing, but the second sentence states that current warming can occur on very slow time scales. U.S. Government
TS	025	18		28	This text should be given more prominence. The role of the oceans in the climate system is critical, but the average layman does not recognize this. Non-technical discussions of climate change focus on the changes in surface air temperature and extreme events. The information in this paragraph is almost never discussed. U.S. Government
TS	025	32		37	Sea level changes are also measured locally against land and so can be affected by land tectonic movements - would be useful to say that here U.S. Government
TS	026	21		22	Either explain how the estimate of carbon uptake by the oceans was derived or delete this statement. The text in section 5.4 says that indirect methods were used, which is obvious since direct measurements were not made since 1750, but does not explain what those methods were. The text in section 7.3 merely restates the quantity of carbon absorbed, and says: "This inventory estimate is currently being revised by several authors." This seems an inadequate basis for inclusion of the information in the TS. U.S. Government
TS	026	33			"...and an uncertainty of a factor of 2..." as a way of describing uncertainty appears here without any previous explanation. Within Chapter 2 this language is used throughout but not within other chapters as much. May want to make the method of describing uncertainty

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					consistent across chapters or at least contain a prefacing discussion in the TS. U.S. Government
TS	026	38		39	Either explain how the estimate of pH change in the oceans was derived or delete this statement. The text in section 5.4 provides an adequate explanation of the way this estimate was derived, but it needs to be repeated here. Clearly, it is not the result of direct measurements made in 1750. U.S. Government
TS	026	49	27	4	Very important – should be emphasized. U.S. Government
TS	027	15			Replace "estimated observed" with "central value for the observed." A value can be estimated or observed, but not both. U.S. Government
TS	028	3		47	The fact that sea level is not equal around the globe is not intuitively obvious to readers who do not work in the area. This fact, and a brief explanation as to why it is a fact, should be presented at the start of this box. A discussion of this topic appears on Pg TS 25, lines 32-34, but it needs to be restated in this box. It would make the explanation of why sea level change is not equal around the globe much more understandable. U.S. Government
TS	028	29	28	33	This needs work in rate determination. U.S. Government
TS	029	7			Change to "Consistency Among Observations in the Last Few Decades." The industrial era is generally taken as beginning in 1750, but none of the information in this section starts with that date. The majority of information is for the late 20th century, with some covering the whole 20th century U.S. Government
TS	029	34		35	Should not contain the words 'believed to be consistent' – they either are or are not consistent.

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
TS	029	40		41	The 3C change applies to the Arctic (not the Subarctic) and the top of the permafrost. U.S. Government
TS	029	45			Sea level rise from 1993-2003 was listed as 3.1 plus or minus 0.8mm on previous pages – here it says 3.0 plus or minus 0.4mm. Should use consistent number. U.S. Government
TS	029	52		57	What is the difference between the two bullets describing evidence for changes in circulation patterns? U.S. Government
TS	031	22		26	Box TS 1.1 provides the obvious definition for "More likely than not," i.e., more than 50% likelihood, but this is too nebulous a definition to be useful. Since "likely" is defined as >66% probability, the implication is that "more likely than not" covers the range 50-66%. If this is the case, it should be clearly stated. If not, some additional information should be provided as to what "more likely than not" means. U.S. Government
TS	031	22		26	Telling a policymaker that something is more likely to have happened, but with only low confidence, which is defined as about 2 out of 10 chances of being correct, is a highly mixed message. Given the low level of confidence, the correct assessment is that it is unknown whether the increase in tropical cyclone intensity can be attributed to human activities. U.S. Government
TS	031	22		26	Box TS 1.1. defines "High confidence" as about 8 out of 10 chances of being correct, and "medium confidence" as about 5 out of 10 chances of being correct. Table SPM-1 introduces a different definition of high confidence and a new term -- moderate confidence. The treatment of uncertainty in this report is difficult enough to follow without changing terminology. The terminology defined in Box TS 1.1 should be used. Amplification of the definitions, i.e., "It is our judgment that this statement has 8 out of 10

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					chances of being correct because ..." would be useful. U.S. Government
TS	031	22		26	The basis for the assessment that it is likely that the increase in warm temperature extremes and decrease in cold temperature extremes is attributable to human activities presented in Table SPM-1, and again on Pg. 11, lines 20-22, is unclear. The underlying text for this topic is section 9.4.3.2, which presents the results of modeling studies which show that including anthropogenic effects "improves the simulation of these changing temperature extremes", but stops well short of attributing those changes to anthropogenic effects. Unless a clear logic can be provided for attributing changes in temperature extremes to human activities, the assessment should be that it is unknown whether these changes can be attributed to human activities. U.S. Government
TS	031	22	31	26	The SPM does not provide a basis, either in Table SPM-1 or in subsequent text, for assigning high confidence to the trends in warm and cool temperature extremes projected for the 21st century. U.S. Government
TS	031	22		26	Change the evaluation of human influence on mid to high latitude cyclones to "unknown." The current evaluation "more likely than not (>50% likelihood) but with low confidence (2 out of 10 chances of being correct)" indicates a high level of uncertainty about the conclusion. Especially in the SPM and TS, WG I has an obligation to be precise about what is known and what is not known. The low level of confidence assigned to this conclusion indicates that it is still unknown. U.S. Government
TS	031	22		26	The definitions of confidence levels given here only add to the confusion over WG I's treatment of uncertainty. High confidence is defined differently from the definition provided in Box TS 1.1, and a new term, moderate confidence, is introduced. Should moderate confidence be considered equivalent to medium confidence, which is defined in

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					Box TS 1.1? Either find different terminology for these terms or use the definitions provided in Box TS 1.1. U.S. Government
TS	031	22		26	The TS does not provide a basis, either in this table or in the text, for assigning high confidence to the trends in warm and cool temperature extremes projected for the 21st century. U.S. Government
TS	032	12		14	The stationary relationships that exist today are used to extrapolate back in time, which also is an important research issue that should be mentioned here. Also, changes to tree-growth (due to CO2 enrichment and nitrogen fertilization) impacts the interpretation of these proxies. Also, in some areas, trees that were previously temperature-limited are becoming more moisture-limited (Briffa et al. 1998). An effort needs to be made to review the potential impact of these changes on the climate-tree growth relationship and how it influences climatic reconstructions. The concern also applies to many other biological proxies. U.S. Government
TS	033	7			Sea-level changes are also measured locally against land, so can be affected by land tectonic movements. U.S. Government
TS	33	43		45	An anthropogenic cause for the circulation change in the atmosphere is suggested but not really proven; note that there has been a recent downturn in the NAO/AO index the past couple of years. A global ocean temperature change had to be available prior to AR4, given that global surface air temperature reconstructions were being performed (and the global warming difference between that in the TAR and that in AR4 is not large). The TAR had already identified changes in Arctic sea ice extent, as well. U.S. Government
TS	033	51	33	51	The term “free atmosphere” needs to be defined.

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
TS	033	56		57	Instead of the awkward phrase 'natural internal variability' why not call it 'unforced variability'? Regardless of what it is called, there is no proof that models produce the observed variability on decadal time-scales because we do not know what the real world values are - the document itself says there is no way to distinguish past 'natural internal variability' from past natural 'externally-forced' variability without a more complete record of past forcings. U.S. Government
TS	033	57	34	1	With that in mind, there is no way to prove that past climate variations have been strongly influenced by external forcings (other than an occasional volcano). U.S. Government
TS	034	1		8	This is a good summary of the uncertainties in the attribution of climate change to human activities and should be retained and strengthened in subsequent drafts. It would be useful to have a table or box highlighting both the reasons for confidence and the uncertainties in attribution of climate change U.S. Government
TS	034	1	34	8	The reasons for confidence and/or uncertainty in the attribution of climate change to human activities are one of the most important results from WG I. We suggest that this information be highlighted in a table or box. U.S. Government
TS	034	2			The uncertainties in the external forcing are not due primarily to uncertainties in the model response, but to lack of suitable observations. In addition, added to the complicating factor for the uncertain model response are the uncertain observations of what really happened. U.S. Government
TS	034	4		7	These lines back up the point made for page 33, lines 56-57; in fact, they make essentially the same point. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
TS	034	12		13	This statement is written awkwardly and is hard to understand. Possible alternative: "It is highly likely that external forcing is needed to explain the warming observed..." U.S. Government
TS	034	26		27	How can the temporal evolution be a key point in understanding the aerosol forcing when we don't know what the aerosol evolution with time has been? U.S. Government
TS	034	39		41	The discussion under the comment in italics does not prove it, or really even comment on the observations. While 'warming' would be expected to decrease sea ice, we don't know how much the warming has influenced the current trend, as the transport influence on Arctic sea ice is undoubtedly a large component of the Arctic sea ice trend. It is uncertain how much of the transport change is anthropogenically induced (the document says a 'part' of it). U.S. Government
TS	034	44	36	10	This is an excellent summary of the strengths and weaknesses of climate models and should be retained in future drafts. U.S. Government
TS	035	32			Modeling of LGM when used with input boundary conditions does not represent much of a test. U.S. Government
TS	036	2		5	This discussion, while theoretically true, is misleading because the AR4 models have not been run as weather forecasting models - so this form of validation is not available. U.S. Government
TS	036	23		25	It is not the ability to simulate temperatures, but to simulate temperatures accurately that can provide evidence; similarly it is not the difficulty in simulating temperatures changes in some parts of the world, but simulating them accurately that is the issue. U.S. Government
TS	037	9		11	This point has already been made on p. 34, lines 39-42.

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
TS	038	41		44	Obtaining quantitative results from EMICs or simpler models is a risky proposition since these models do not solve the full conservation equations. The ability to mimic the GCM results does not imply that the answers are being obtained for the right reason, nor that answers obtained with changing scenarios or parameters will be similar to what GCMs would produce. To the extent that conclusions are drawn from their quantitative, as opposed to qualitative, results, the level of confidence should be reduced accordingly. (Note that in chapter 8, the first requirement on p. 91 to have confidence in models is that they solve the full conservation equations.) U.S. Government
TS	039	43	40	19	This whole definition of 'commitment' is not really well founded, at least use the twenty-year old definition of 'unrealized warming' instead of inventing a new term. Also this is not the true commitment (per dictionary definition) as it does NOT include the commitment of current power plants, etc. Please see notes on SPM, this is a poorly formed idea, and does not help, or indeed misleads, the governments in understanding our commitment to climate change. U.S. Government
TS	039	50	39	50	The statement that the deep ocean has response time scales longer than 1000 years is not true particularly in the North Atlantic and should be modified to indicate regional differences in these response times. U.S. Government
TS	040	27			Must include/discuss the projection of rapid short term ice sheet changes. U.S. Government
TS	040	39			Please be specific, not 'several' meters, but 4 to 6 ? U.S. Government
TS	041	37			This, of course, assumes that climate sensitivity does not change with climate change, is this possibly important, does it need a caveat?

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
TS	042	38			Is there to be a discussion of regions where the warming is not well-constrained by models (e.g., tropics)? U.S. Government
TS	042	52			The definition of “heat wave” (e.g., degrees above normal) is needed. U.S. Government
TS	042	56		57	“except where surface properties change” – Is this related to terrain height above sea level, oceans, and associated qualities? U.S. Government
TS	043	2		4	Should define what is meant by “extreme warm seasons” as it’s a little confusing that the probability in the tropics increases by 90% compared to 40% elsewhere when places “elsewhere” are projected to warm more. Should clarify that “extreme warm season” refers to surpassing some threshold average temperature, if that’s in fact how it’s defined. U.S. Government
TS	043	28		45	The significance of an MOC slowdown in models should be given (i.e., does a 60% change impact simulated atmospheric climate). Since Bryden et al. claim a 30-50% slowdown in the MOC, is there any observational evidence for observed atmospheric changes. The emphasis on the MOC throughout the ocean section needs such a “reality check”. U.S. Government
TS	044	10		16	The modeled increase in ENSO response patterns is not consistent with increased hurricane intensity as one feature associated with ENSO is increased wind shear over the Atlantic, which tends to decrease hurricane intensity. This apparent inconsistency needs to be addressed. U.S. Government
TS	044	43	44	48	Insert at the beginning of the sentence: “Although Antarctica appears to have lost mass during the last decade, during the next century, Antarctica will gain mass...” The report needs the TS to reconcile the statement that Antarctica will gain mass in the

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>next century, with the discussion on page 24 which says that Antarctica lost mass over the last decade. This paragraph appears to be an attempt to explain why the recent observations don’t imply that the contribution from Antarctica could be either positive or negative in the next century. But it is so vague—and the average reader does not know what you mean by recent dynamical imbalance.</p> <p>Many scientists who do not follow these matters as closely as the authors of chapters 4 and 10—not to mention the public—are under the impression that the recent positive contribution from Antarctica would imply that the future contribution may be positive—or at least that we simply do not know whether the positive or negative factors are strongest. In writing this passage, the authors need to recognize this presumption that most readers have. To say that, in spite of the recent positive contribution, the future contribution will be negative—rather than uncertain—requires a persuasive explanation that simply is not in this summary right now. In an ideal world, such an analysis might be in Chapters 4 and 10—but this is really an issue that cuts across both chapters and hence it may be up to the TS authors to work this out.</p> <p>U.S. Government</p>
TS	044	50	44	52	<p>Please explain why IPCC expects sea level rise to decelerate. This passage seems to suggest a rate of 2.5 mm/yr in the next 50 years and 2.9 mm/yr in the next century. But on page 27 the TS says that sea level is currently rising 3.1 mm/yr. Much of this IPCC report provides reasons for why we might expect the sea to rise more rapidly in the future—including text that suggests that there has been some recent acceleration. Simply providing a projection without analysis is not enough—especially when the projection shows the opposite of what one would otherwise expect.</p> <p>U.S. Government</p>
TS	048	35			<p>The lack of mention of surface, tropospheric, and stratospheric temperature trends is surprising, especially given the focus on this topic earlier in the TS and in Ch. 3. Given the major new works on upper tropospheric trends, one would expect this to be a major, robust</p>

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					finding. U.S. Government
TS	049	20		21	The 3C statement is based on specific regions, this statement now extends it globally. Better to insert at start: In specific regions permafrost temperature have warmed etc. As for use of the word thinning, better to rephrase and say “less deep” U.S. Government
TS	049	42			Why freshening at the equator if precip has decreased as low latitudes? U.S. Government
TS	051	4			A 'substantial' fraction is vague; it should be noted that a more precise determination is limited by our lack of knowledge of the past radiative forcing variations associated with both volcanoes and solar irradiance variations. U.S. Government
TS	051	48			How about ENSOs? U.S. Government
TS	052	42			Tropical precip forecast to increase, opposite to what has been happening... This points to a serious problem with "robust findings" about projections that make no sense when compared with current trends – THIS NEEDS TO BE HIGHLIGHTED, and possibly the robustness reduced(?) U.S. Government
TS	052	51		53	If not 2100 then when? U.S. Government
TS	053	37		46	Surprised by the level of confidence expressed here – i.e. very likely or 90-99%. This is the same level of confidence assigned to the fact that most of the warming of the last 50 years is mostly attributable to human activities. Are we really objectively as confident about regional precipitation projections of the future as we are about attribution of recent warming? U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
TS	059				SPM-2 and TS-5 figure needs more explanation in caption describing how forcing is defined, relative to what, and how natural and anthropogenic differentiated. Fig 3 from IPCC 2001 was much more defined more carefully and accurately. U.S. Government
TS	064				The figure caption makes reference to T2 and T12. These terms need at least a little context, as far as saying, “These are IR channels on satellite X, instrument Y,” and possibly specify wavelengths. U.S. Government
TS	065				The upper panels of this figure need to be larger. U.S. Government
TS	066				The lower panel seems to be missing from this figure. U.S. Government
00					SPM, TS, 3,4,5: The AR4 should articulate the fact that the majority of observational results presented are based on data not designed for climate monitoring and not meeting the UNFCCC-adopted climate monitoring principles. It is important for governments and the public to be reminded of this major problem in climate science. U.S. Government
00					(On chapters 2-5) The “Observations” chapters would be improved, in my opinion, if they had a consistent format. While all chapters do a good job of discussing observed changes, some chapters include a discussion of causes for those changes (as best we know them), some discuss the consequences of the changes, and some include a list of the highest priority observations and research topics that are needed to pin down and understand the changes. I think all of these chapters should have the same set of sections See also general comment against assigning attribution in the observations chapters and recommendations for reducing their length (the “bubble up” comment from the observations group). U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
00					<p>Scientific convention is to define uncertainty range as +/- two standard deviations (95% confidence interval). This is acknowledged in Box TS 1.1., Pg TS 4, lines 41-42, where the +/- two standard deviations is given as the default range, and in Chapter 3 (Pg. 3-7, lines 18-19), where the authors use +/- two standard deviations because "This allows us to assess what is really unusual." The use of +/- one standard deviation for uncertainty range in some cases (e.g. for RF) is misleading because it cuts the uncertainty range in half. It is also confusing because in most other places in this report (e.g. Chapter 3) the conventional approach of using +/- two standard deviations is used. The conventional approach should be used through out the report and all uncertainty ranges stated as +/- two standard deviations</p> <p>U.S. Government</p>
00					<p>The Italicized pg attempts to provide a concise answer to the end of chapter general questions (Frequently Asked Questions, FAQs). Where this italicized response is incorrect or incomplete it should be corrected or completed. The chapters should reconsider, take more time and care, on the choices of the end of chapter questions. They may not all be the most likely FAQs of Policy/Decision stakeholders. This has the potential of being one of the high impact parts of the assessment, if needed expertise and effort is provided. Also, be clear about the criteria for the questions that were chosen. It appears that some of the answers do not always seem to be fully based on the text of the chapter from which they presumably should be drawing their response. For example, the response to question 10.2 includes definitions of the words "abrupt" and "major." However, box 10.1 at p. 10-69 of the text of Chapter 10, which is titled "Future Abrupt Climate Change, 'Climate Surprises' and Irreversible Changes," does not define the words "major" or "abrupt." The text instead includes a "working definition" of the phrase "abrupt climate change," which differs from the definition of the word "abrupt" that is defined in the reply to the question. However, the reply defines both words. In fact, the reply to the question states that "there is no rigorous definition" of the terms "abrupt" and "major." Nevertheless, the reply goes on to say that "'abrupt' conveys the meaning that the changes occur much faster than the</p>

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>perturbation which is inducing the change.” It then states, “In the case of greenhouse gases, which increase on a time scale of several decades to centuries, <u>‘abrupt’ would denote changes that evolve in a few decades or less</u>” (p. 10-96) (emphasis added). It gives as an example, the possible changing of “course” of the “extension of the Gulf Stream in the Atlantic Ocean,” and states that such change “would clearly be considered an abrupt change.” However, while the underlying text (p. 10-69) discusses that climate models since the TAR “have provided a more detailed view on the anticipated changes of the Atlantic meridional overturning circulation (MOC) in response to global warming,” that text, unlike the answer to the question, adds, “There is no direct model evidence that the MOC could collapse within a few decades in response to global warming.”</p> <p>U.S. Government</p>
00					<p>The report offers a generally very good assessment of the state of scientific knowledge regarding climate change. At times, it also points to additional needed research, although this is less clear. It would be very helpful if the report could include a Table identifying research issues, the uncertainty surrounding the issue, and the level of urgency of addressing the issue. Have future assessments look back to measure benefit of response, if appropriate.</p> <p>U.S. Government</p>
00					<p>The subsections labeled “robust conclusions and key uncertainties” are very important. Never lose this perspective from this or future assessments.</p> <p>U.S. Government</p>
01	00				<p>The chapter ends in an abrupt manner and lacks a clear, unifying summary. Suggest including such a summary, highlighting the main points of the discussion. Is the Technical Summary intended to do this? If so state clearly and cross-reference.</p> <p>U.S. Government</p>
01	00				<p>As we progress from history to the present time, Chapter 1 should include linkages to the</p>

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					following Chapters in AR4. Chapter should end with an introduction into the key issues that inspired research and discussions for the AR4. U.S. Government
01	00				The historical overview is an excellent and idea, and a well-written addition. The links between this chapter and the subsequent chapters, such as on p. 12, line 38, are especially useful. This was done on this page, but more links to subsequent chapters would be helpful. These not only should appear in the text, but should appear in the summary (to be written). This would provide the reader with a roadmap that connects past work with AR4. Topics could include “Moving beyond the Hockeystick” and “Reconciling surface and upper-air temperature trends”. U.S. Government
01	001		26		In Chapter 1, p. 1-26, there appears “Box 1.1: Treatment of Uncertainties in the Working Group I Assessment.” A very similar Box “TS.1.1” appears in the TS, p. TS-3. Is the box needed in both places? And, if so, why are there some wording differences between them? U.S. Government
01	001	11			Change Ronald Stouffer to Ronald J Stouffer (or R. J. Stouffer). Please. U.S. Government
01	003	18	5	2	The philosophical discussion of the nature of science is very interesting and offers some useful insights. However, the authors should take the further step of trying to define what these characteristics of research might mean to a policymaker trying to extract advice from ongoing scientific research. Authors should consider whether this fits their view of the chapter’s goals. U.S. Government
01	003	52		54	What is the point of this sentence mentioning a few unusually gifted scientists? Delete entire sentence. U.S. Government
01	004	04		6	The role of IPCC reports in stimulating and focusing climate change science is a bit

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					understated here. Scientists are always looking for “official” guidance and vindication of their research. You often see statements in IPCC reports cited on the front end of research proposals as justification for the Principal Investigator(s) going after some specific gap in knowledge. This particular sentence should be preceded by another statement that says the following: “While specific research directions are often prescribed by granting agencies, individual scientists exercise considerable latitude in selecting research questions and strategies. After all, science is a competitive enterprise in which detailed proposals are peer-reviewed and ranked by expert panels; the best ideas and most competent research teams normally merit funding. Judging from repeated citations to its findings and recommendations in both research proposals and the scientific literature, the IPCC has stimulated and coordinate targeted research to answer important climate change questions.” U.S. Government
01	004	12	4	12	Replace “might ideally be required” to “will be required”. This seems obvious. U.S. Government
01	005	04	9	14	The discussion in Section 1.3 is much improved over the first-order draft. However, it still fails to acknowledge any drawbacks in the IPCC process. The IPCC has achieved tremendous progress. However, the process of consensus and Government approval generally leads to a “lowest common denominator” product. Also, the timelines are such that products are often dated almost before they are published. For example, although the Aviation and the Global Atmosphere report quoted was published in 1999, the process is such that the final product was based on early to mid-1990s research. Given the scientific advances in the last decade, many of the findings are now dated. However, in the absence of a new consensus report, the 1999 report continues to exert substantial influence, even though arguably its relevance is diminishing. Making these points will provide important context for policymakers. U.S. Government

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01	005	12			The fact that mixing in the atmosphere allows extrapolation of the Mauna Loa point results to a global perspective should be made here. U.S. Government
01	007	28			The discussions in Chapters 1 and 3 are not detailed enough to do the urban heat island effect justice. Needs a more organized discussion about the questions raised and how addressed/resolved. For example, one of the main findings in Chapter 3 is that there have been increases in the extremes of temperatures, which are consistent with global warming. In an analysis of Australian and Argentine temperatures, Camilloni and Barros (1997) showed that interannual variability of temperature is generally lower in urban environments than in rural areas; in other words, urban stations are prone to have lower trends in absolute value than rural ones. Could the trend in temperature extremes globally simply reflect a disproportionate increase of rural stations globally over time? The scientific consensus is that in a global analysis, such biases all tend to come out in the wash. Consider merit of including Camilloni and Barros findings to the discussion. U.S. Government
01	007	51			Should also include Argo floats, which provide SST observations with over 2000 now deployed. Change “several hundred” to “several thousand”. U.S. Government
01	007	52			Typo: <i>In situ</i> in <i>situ</i> should just be <i>in situ</i> U.S. Government
01	008	49		50	A better sentence would read, “...warming in 1980, Madden and Ramanathan (1980) and Hansen et al. (1981) predicted it would be evident by the year 2000.” (The Madden-Ramanathan text p. 767 says it would appear sometime before 2000, and Hansen et al. similarly but independently, using different methods.) The reference is: Hansen, J. E., D. Johnson, A. Lacis, et al., 1981: Climate impact of increasing atmospheric carbon dioxide. <i>Science</i> , 213, 957-966. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
01	009	06			A number of recent publications by Barnett, Levitus, and colleagues have “fingerprinted” the anthropogenic signal in ocean temperature data. Cite them. U.S. Government
01	009	21			If a hypertext reference is permitted, replace “Fleming (1998) provides” with “Fleming (1998) and Weart (2006) provide...” The new reference is: Weart, S., 2006: <i>The discovery of global warming</i> , http://www.aip.org/history/climate/ . This work has been peer-reviewed so far as is feasible for an annually updated web site. It comprises about a quarter of a million words and 1700 references on the topic. If hypertext referencing is not allowed, a possible substitute would be Weart (2003): Weart, S., 2003, <i>The discovery of global warming</i> , Harvard University Press, Cambridge, MA, 228 pp. U.S. Government
01	011	13	11	13	Abrupt climate change is mentioned often in the text. The term should be defined even if only a range can be given based on previous paleoclimate studies. Use definition from Chapter 6, page 18, lines 14-17. U.S. Government
01	012	01		26	Text repeats itself with some variations; the two passages should be combined. U.S. Government
01	012	12		13	Pollen is not the only kind of plant evidence, oxygen isotopes are not the only isotope, varved lakes are not the only kind of lake sediment, and loess not the only kind of terrestrial sediment that contributed to paleoclimate reconstructions in the FAR. Sentence should be amended to read, “Paleoenvironmental/paleoclimatic reconstructions cited in the FAR were based on various kinds of data, including pollen and plant macrofossil records, insect and animal remains, growth and geochemical measurements from tree rings, corals and ice cores, and other sedimentological and geochemical data from lake, terrestrial, and marine sediments. These records provided estimates of climate variability on time scales from millions of years to the last few centuries, in some settings at year-to-year resolution

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					(e.g., laminated marine and lake sediments, tree rings, corals, speleothems, and ice cores).” U.S. Government
01	012	16			Cite the authors for “FAR chapter 2”. While this appears on page 14, not 16, citation of earlier ARs should be consistent throughout AR4. U.S. Government
01	013	45	14		This chapter overlooks work before the FAR on non-CO ₂ greenhouse gases. It would be worth at least mentioning that already in the 1970s, Veerabhadran Ramanathan and others noted that some gases were more effective in trapping radiation than CO ₂ , and their levels were rising rapidly. Good references would be: Ramanathan, V., 1975, Greenhouse effect due to chlorofluorocarbons: Climatic implications, <i>Science</i> , 190, 50-52 Ramanathan, V., L. B. Callis, and R.E. Boughner, 1976, Sensitivity of surface temperature and atmospheric temperature to perturbations in the stratospheric concentration of ozone and nitrogen dioxide, <i>J. Atmospheric Sciences</i> , 33, 1092-1112. U.S. Government
01	014	12			The reference to the chapters seems unclear. Perhaps say “For more details see chapters 2, 7, and 10.” This comment also applies to lines 29 and 43 and later. U.S. Government
01	016	37			<i>In situ</i> <i>in situ</i> should just be <i>in situ</i> . Same problem on line 51. U.S. Government
01	017	43		44	Suggested replacement: “Stommel (1961) proposed a mechanism, earlier suggested by Chamberlin (1906)...” where the reference is: Chamberlin, T. C., 1906: On a possible reversal of deep-sea circulation and its influence on geologic climates. <i>J. Geology</i> , 14, 363-373. U.S. Government
01	017	55	18	8	The discussion of coupling between air and sea and its role in climate applies primarily to the extra tropical regions as we have a better idea of the effects of coupling on climate in

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					the tropics (e.g., ENSO as discussed in the following paragraph). This distinction should be made in the text. U.S. Government
01	019	26	24	2	Section 1.5 lacks any clear conclusions and themes. Suggest at the very least trying to answer “what is the cost-benefit” of moving between hierarchies of models. U.S. Government
01	019	49			“More realistic” is too subjective. Suggest providing an illustrative example that provides some degree of quantification. U.S. Government
01	020	43			Delete “the transport of” U.S. Government
01	021	20	21	21	Add Manabe and Wetherald 1975 to reference list. U.S. Government
01	022	21			The slab models omitted all CHANGES in ocean dynamics. U.S. Government
01	022	24		28	Is this statement really true? There are still questions of whether coupled models have improved our climate modeling capabilities. U.S. Government
01	022	39			Add Manabe and Stouffer 1988 to flux adjustment list Sausen et al and M+S developed flux adjustments are the same time. U.S. Government
01	022	39		42	Change flux corrections to flux adjustments. This is the common usage of the term. U.S. Government
01	022	48		57	Need to add reference and discussion from Stouffer and Dixon 1998. They develop a nice framework for discussing the initialization problem in AOGCMs. U.S. Government
01	022	57	23	2	Isn’t an <i>ad hoc</i> tuning of radiative parameters a form of “flux adjustment”? If so, this

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					sentence should be modified to indicate this fact. U.S. Government
01	023	12			Add “so strongly” after “second kind do not depend”. There are cases of multiple equilibria arising from different initial conditions in climate models. Eq. M+S 1988. U.S. Government
01	024		25		This section specifies when and where the government plenaries for the SAR (Madrid 1995) and TAR (Shanghai 2001) were held, but not the FAR. For consistency add the date and place of the FAR. By the same token, the report identifies Bert Bolin and John Houghton as the leadership for the FAR, but does not identify the leadership of the SAR and TAR. U.S. Government
01	024	18		19	The IPCC syntheses have inspired scientific research leading to new findings, so point to one or two prime examples from the first three reports. This could be incorporated into the discussion of findings and oversights for each of the three reports on p. 24-26. Include a paragraph on p. 25, before line 12, that would describe how each IPCC report stimulated new research, ending with three or four nuggets of how the Fourth Assessment Report could affect the course of climate change science in the near future. U.S. Government
01	024	37			Change to “The WG1 FAR ...” The FAR consisted of three volumes, and it is incorrect describe it as consisting of only WG1’s report. U.S. Government
01	024	42			Reference to section 7.1 is lengthy and on line 46. U.S. Government
01	031	23			Incorrect reference. Harriss’s web site gives: Harriss, R.C., K. Bartlett, S. Frolking, and P. Crill, 1993, Methane emissions from northern high-latitude wetlands, in R.S. Oremland (ed.), Biogeochemistry of Global Change, N.Y.: Chapman & Hall, pp. 449-486.

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					U.S. Government
01	037	37		43	The <i>direct</i> effect of solar variation, through changes in total irradiance, is known to be small. Thus page 1-13, at lines 37-43, notes that if solar variation is to be a significant contributor to observed variations in global temperature, it must be through "unknown large feedbacks in the climate system," such as effects on cloud nucleation. It goes on to note that mechanisms by which such indirect solar effects are at this point highly speculative and require more research. What is missing in this section is any acknowledgment of the large body of evidence that SOME kind of indirect mechanisms must be at work, given the numerous findings of strong correlations between solar activity and global temperature in the historic and geological records. U.S. Government
01	044				Figure 1.2 is much improved from the first-order draft, yet still hard to interpret. Could some more text explaining the evolution be inserted? Also note what else is lacking in the models. Figure 1.2 either needs to be dramatically improved or deleted. U.S. Government
01	044				Along with the increasing complexity, could an assessment of improvements in uncertainty be included in Figure 1.2? U.S. Government
02	00				It is stated in Box TS 1.1 (page TS-4, lines 41-42) that \pm two standard deviations is the default range "where values are specified . . . as a central estimate with a plus/minus range", and in Chapter 3 (page 3-7, lines 18-19), where the authors use \pm two standard deviations because "This allows us to assess what is really unusual." However, radiative forcing (and other variables?) in general seems to make use of 1-sigma. A consistent approach should be used through out the report and it should be made clear in the chapter when the approach differs or whenever one sigma is used. Also, to add to the confusion, in some cases in this chapter [e.g. for radiative forcing (RF – Page 21, line 43)], the

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					uncertainty range is given as \pm one standard deviation with an additional, unexplained range that is asymmetric about the centroid. U.S. Government
02	00				Organizations and government agencies periodically are reorganized or change names. The latest names should be reflected in this report. An example is periodic references to “CMDL”, a former laboratory in NOAA. This now should be referenced as the NOAA Earth System Research Laboratory (NOAA/ESRL) or simply NOAA if the ultimate source is unambiguous. There are likely others. U.S. Government
02	002	03			In each chapter or major section a clear reference should be made at (1) the first occurrence of a LOSU term (“medium,” etc) and (2) a qualitative term of uncertainty (“very likely,” etc) directing the reader to the detailed description of these sets of terms and/or to the glossary. U.S. Government
02	003	5	9	10	The authors recognize that the use of RF to measure climate change is limited as it does not represent the overall climate response. This is especially true for absorbing aerosols. Black carbon, like greenhouse gases has positive forcing at TOA but opposite from greenhouse gases has a negative surface forcing. Therefore absorbing aerosols may significantly perturb surface temperature and other climate variables even in the case of zero RF. For absorbing aerosols, the surface forcing is needed to evaluate climate response. Therefore, the panel suggests 1) adding at the end of the second sentence of the executive summary: “an exception is absorbing aerosols” 2) adding Page 5, Line 43: “...and potentially surface temperature.” 3) replace Page 7, line 14-15 by the sentence on Page 61, Line 27-29 which mentions than both RF and surface forcing are important to evaluating climate response, and should not

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					be directly compared 4) adding to Page 9, Line 10, a sentence to note the limitation of RF in the case of absorbing aerosol, for which the impact on climate should rather be measured in terms of surface forcing. U.S. Government
02	004	41			An explanation for the asymmetrical uncertainty band needs to be provided. Here and elsewhere, any uncertainty that is not the default ± 2 -sigma requires an explanation or cross-reference. U.S. Government
02	005				There are three conclusions about radiative forcing from aerosols and associated uncertainty that appear on pages 2-5 and are elaborated later in the chapter: 1) A combined total direct aerosol RF is given as $-0.5 \pm 0.4 \text{ Wm}^{-2}$, with a low level of scientific understanding... 2) The cloud-albedo RF due to aerosols (also referred to as first indirect or Twomey effect) is estimated to be $-0.9 \pm 0.5 \text{ Wm}^{-2}$, with a very low level of scientific understanding. Other processes related to aerosol-cloud interactions remain highly uncertain and there is a very low level of scientific understanding of these processes... 3) Observations and models indicate that both the direct effect of aerosols and aerosol-cloud interactions lead to a substantial reduction of shortwave radiative flux at the surface which alters the surface heat and moisture budgets... At the suggestion of one of the authors of Chapter 2, several scientists undertook a detailed elicitation of 24 experts about uncertainty in radiative forcing by aerosols. The paper was peer reviewed and accepted for publication in <i>Climatic Change</i> before the literature cut-off date. (Granger et al, 2006, Elicitation of expert judgment of aerosol forcing, (<i>Climate Change DOI: 10.1007/s10584-005-9025-y</i> .) We recommend that IPCC compare their uncertainties in RF to the uncertainties reported in this paper, which used an independent approach.

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					U.S. Government
02	006	19			The phrase, "Since 1750, humans have <i>very likely</i> exerted a net warming influence on climate." should be immediately followed by the statement: "Moreover, it is <i>likely</i> that this warming influence exceeds +0.8 W/m ² , which has been suggested as a minimum threshold for any combination of natural and anthropogenic forcings to be capable of explaining the observed, industrial-era increase in global-mean surface temperature." U.S. Government
02	006	23			Although it is not necessary to provide detailed explanations in the chapter summary bullets, it is not clear here or in the text how RF values have been weighted by "level of scientific understanding". It should be made clear exactly how this was done. Was there an additional weighting other than the size of the uncertainty? – i.e., in Figure 2.24 (p. 158), the top panel suggests that this might be true, whereas the bottom panel indicates that errors were applied equally to expand the total error after adding aerosols and other components with low level of scientific understanding. U.S. Government
02	006	27		50	Mention is made of the 11-year solar cycle, but it is well known that every other cycle is different. This, in effect, results in a 22-year cycle. One has to look at changes from one 22-year period to the next to ascertain changes in the solar irradiance. The double cycle is due to the reversal of the solar magnetic field. This has technical merit. Consider it if it makes any difference. U.S. Government
02	007	2			The influence of volcanic aerosols on the <i>radiative</i> energy budget may be transitory, but that effect is translated into the ocean heat budget and it lasts for quite a long time there (decades to centuries). Thus, this statement seems a bit misleading in how it describes the impact of vulcanism on the Earth's energy balance. U.S. Government

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02	007	17			Insert "aerosol" between "mean" and "surface" U.S. Government
02	014	9		15	There is new work since the TAR on bottom-up methane emission inventories and projections. The authors should consider National Communications submitted to the Climate Secretariat (these are official inventories), available at www.unfccc.net . Also, a new EPA draft report is available at: http://www.epa.gov/nonco2/econ-inv/international.html , which compiles many of these National Communications, containing current and projected (out to 2020) methane emissions done with bottom up inventory methods. U.S. Government
02	019	48	20	7	This paragraph is too complicated, and needs a synthesis or assessment statement. If the papers discussed in this paragraph represent a very uncertain state of knowledge, then consider to delete the paragraph, or at least write a more concise, shorter paragraph. U.S. Government
02	024	51			Sentence needs to be reformatted. U.S. Government
02	029	17	45	37	A significant omission from this section is any discussion of the development of global aerosol microphysics models for predicting aerosol size distributions and, therefore, cloud condensation nuclei (CCN) concentrations. This has been a widespread activity pursued by many research groups since the IPCC TAR. Such a discussion could logically fit into one of several sections: Section 2.4.4 "Advances in Modeling", Section 2.4.6.2 "Estimates of the RF due to albedo effect from GCMs", and/or Section 2.4.6.5 "Uncertainties in RF due to model biases". For example, Section 2.4.6.5 (page 2-45, line 6) cites Feingold (2003), which demonstrates how aerosol size distribution is a significant determinant of the cloud albedo effect. It seems appropriate to mention the considerable resources that have been dedicated to developing a prognostic representation of the aerosol size distribution in global models.

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					<p>As rightly pointed out, the majority of radiative forcing estimates invoke empirical parameterizations of the relationship between cloud droplet number concentration (CDNC) and aerosol (often sulfate) mass (e.g. Boucher and Lohmann, 1995). A weakness of this approach was first pointed out by Kiehl et al. (2000), where it was shown that a variety of such empirical relations have been proposed and give significantly different estimates of the cloud albedo effect. Given the fact that such variability in the empirical relationship is likely real (i.e. not measurement error), they suggested taking a more mechanistic approach to predicting CCN in global models. A further drawback of the empirical approach is that aerosol mass is a poor surrogate for CCN concentrations. For example, using a global aerosol microphysics model, Adams and Seinfeld (2003) found differences of a factor of two in CCN concentrations between two simulations with (essentially) the same aerosol mass concentrations because of different underlying aerosol microphysical processing. A related disadvantage of the empirical approach is that the aerosol microphysical pathways that produce CCN are treated implicitly, making the method something of a “black box”. While such simplicity may be useful in many applications, explicit aerosol microphysics allows one to single out specific physical mechanisms and their uncertainties (e.g. nucleation) in sensitivity tests to evaluate how such uncertainties translate into uncertainties in the indirect effect itself.</p> <p>As an indicator of the amount of work in this area, I am including a (probably incomplete) list of publications related to global aerosol microphysics modeling since the TAR. These include papers discussing model development, application to the indirect effect, and algorithm intercomparison. Note that few, if any, of these publications were published in time to be cited in the IPCC TAR report, so that this indeed represents a new trend in global aerosol modeling.</p> <p>Adams, P.J., and J.H. Seinfeld, Predicting global aerosol size distributions in general circulation models, <i>Journal of Geophysical Research</i>, 10.1029/2001JD001010, 2002.</p> <p>Adams, P.J., and J.H. Seinfeld, Disproportionate impact of particulate emissions on global cloud condensation nuclei concentrations, <i>Geophysical Research Letters</i>,</p>

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					<p>10.1029/2002GL016303, 2003.</p> <p>Boucher, O., and U. Lohmann, The Sulfate-Ccn-Cloud Albedo Effect - a Sensitivity Study with 2 General-Circulation Models, <i>Tellus Series B-Chemical and Physical Meteorology</i>, 47 (3), 281-300, 1995.</p> <p>Easter, R.C., S.J. Ghan, Y. Zhang, R.D. Saylor, E.G. Chapman, N.S. Laulainen, H. Abdul-Razzak, L.R. Leung, X.D. Bian, and R.A. Zaveri, MIRAGE: Model description and evaluation of aerosols and trace gases, <i>Journal of Geophysical Research-Atmospheres</i>, 109 (D20), 2004.</p> <p>Ghan, S.J., R.C. Easter, E.G. Chapman, H. Abdul-Razzak, Y. Zhang, L.R. Leung, N.S. Laulainen, R.D. Saylor, and R.A. Zaveri, A physically based estimate of radiative forcing by anthropogenic sulfate aerosol, <i>Journal of Geophysical Research-Atmospheres</i>, 106 (D6), 5279-5293, 2001.</p> <p>Gong, S.L., and L.A. Barrie, Simulating the impact of sea salt on global nss sulphate aerosols, <i>Journal of Geophysical Research-Atmospheres</i>, 108 (D16), 2003.</p> <p>Gong, S.L., L.A. Barrie, J.P. Blanchet, K. von Salzen, U. Lohmann, G. Lesins, L. Spacek, L.M. Zhang, E. Girard, H. Lin, R. Leaitch, H. Leighton, P. Chylek, and P. Huang, Canadian Aerosol Module: A size-segregated simulation of atmospheric aerosol processes for climate and air quality models - 1. Module development, <i>Journal of Geophysical Research-Atmospheres</i>, 108 (D1), 2003.</p> <p>Herzog, M., D.K. Weisenstein, and J.E. Penner, A dynamic aerosol module for global chemical transport models: Model description, <i>Journal of Geophysical Research-Atmospheres</i>, 109 (D18), 2004.</p> <p>Jacobson, M.Z., Analysis of aerosol interactions with numerical techniques for solving coagulation, nucleation, condensation, dissolution, and reversible chemistry among multiple size distributions, <i>Journal of Geophysical Research-Atmospheres</i>, 107 (D19), 2002.</p> <p>Jung, C.H., Y.P. Kim, and K.W. Lee, Multicomponent aerosol dynamics model with gas/particle transport and modal approach, <i>Environmental Engineering Science</i>, 21</p>

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>(4), 437-450, 2004.</p> <p>Kiehl, J.T., T.L. Schneider, P.J. Rasch, M.C. Barth, and J. Wong, Radiative forcing due to sulfate aerosols from simulations with the National Center for Atmospheric Research Community Climate Model, Version 3, <i>Journal of Geophysical Research-Atmospheres</i>, 105 (D1), 1441-1457, 2000.</p> <p>Pierce, J.R., and P.J. Adams, Global evaluation of CCN formation by direct emission of sea salt and growth of ultrafine sea salt, <i>Journal of Geophysical Research</i>, 111, 10.1029/2005JD006186, 2006.</p> <p>Rodriguez, M.A., and D. Dabdub, A modeling study of size- and chemically resolved aerosol thermodynamics in a global chemical transport model, <i>Journal of Geophysical Research-Atmospheres</i>, 109 (D2), 2004.</p> <p>Spracklen, D.V., K.J. Pringle, K.S. Carslaw, M.P. Chipperfield, and G.W. Mann, A global off-line model of size-resolved aerosol microphysics: I. Model development and prediction of aerosol properties, <i>Atmospheric Chemistry and Physics</i>, 5, 2227-2252, 2005.</p> <p>Stier, P., J. Feichter, S. Kinne, S. Kloster, E. Vignati, J. Wilson, L. Ganzeveld, I. Tegen, M. Werner, Y. Balkanski, M. Schulz, O. Boucher, A. Minikin, and A. Petzold, The aerosol-climate model ECHAM5-HAM, <i>Atmospheric Chemistry and Physics</i>, 5, 1125-1156, 2005.</p> <p>Vignati, E., J. Wilson, and P. Stier, M7: An efficient size-resolved aerosol microphysics module for large-scale aerosol transport models, <i>Journal of Geophysical Research-Atmospheres</i>, 109 (D22), 2004.</p> <p>Wilson, J., C. Cuvelier, and F. Raes, A modeling study of global mixed aerosol fields, <i>Journal of Geophysical Research-Atmospheres</i>, 106 (D24), 34081-34108, 2001.</p> <p>Zhang, Y., R. Easter, S. Ghan, and H. Abdul-Razzak, Impact of aerosol size representation on modeling aerosol-cloud interactions, <i>Journal of Geophysical Research</i>, 107 (D21), Art. No. 4558, 2002.</p> <p>U.S. Government</p>

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02	030	08			Specify the wavelength associated with the optical depths in the sentence. U.S. Government
02	030	34		35	Define what is meant by “structural uncertainty”. Add “structural uncertainty” and “value uncertainty” to the Glossary and refer to the Glossary when those terms are first used in the chapter. U.S. Government
02	034	07		12	Agree with the authors that biomass aerosol emissions are essentially uncontrolled, and have less potential for coming under control compared to other aerosol emission sources. However, it would still be useful for IPCC to break out, if feasible, the separate BC and OC radiative forcing associated with biomass burning. This could be one estimate in a table, rather than keeping track of the separate forcings all throughout the report. U.S. Government
02	035	20			In Section 2.4.5.5 add a statement about the consequences for radiative forcing of the chemical interaction between sulfate and nitrate: Nitric acid will not form ammonium nitrate aerosol unless the sulfate is fully neutralized. This has two consequences for aerosol radiative forcing. First, radiative forcing by nitrate aerosol is often sensitive to emissions of ammonia as well as emissions of NO and other precursors of nitric acid. Emissions of ammonia are expected to increase [7.4.2.1]. Second, the radiative forcing from reductions in sulfate (e.g. by control of SO ₂ emissions) will be partially compensated in some regions by increases in nitrate (West et al., Marginal direct climate forcing by atmospheric aerosols Atmos. Environ., 1998; Liao and Seinfeld, 2005). U.S. Government
02	036	32			Define what is meant by “diversities”. U.S. Government
02	043	29			Define what is meant by “subcloud” or use a better phrase, i.e., “below the cloud”. U.S. Government
02	044	5			Replace “of the aerosol indirect effect” with “of the derived aerosol indirect effect”. The

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					current sentence implies that the actual aerosol indirect effect is dependent upon the spatial resolution of models and observations. U.S. Government
02	051	3		13	Text notes that only contrails and cirrus are directly addressed for aviation because other impacts are included earlier (sections 2.3 and 2.4). There is no explicit discussion of aviation in these sections and contrails/cirrus impacts may be relevant to the broader climate change policy discussion. U.S. Government
02	051	12		13	The phrase "Aviation aerosol also can potentially alter the properties of clouds that form later in air containing aircraft emissions." contradicts the earlier statement that effects of aviation emissions that are not specific to just aviation (such as aerosols) are discussed in other sections. Aviation aerosols need to be considered in context of all other aerosol sources. What % comes from aviation? U.S. Government
02	051	24			Using "flight regions" is much better than the previous "flight tracks". However, "remain unchanged" needs to be clarified -- remains unchanged from what? (note that regions or tracks are not perfectly static so it is important to clarify what the authors mean). For example, what are some of the important parameters (e.g. meteorology impacts on fuel burn). U.S. Government
02	051	28			Insert "linearly" before "scaled". U.S. Government
02	052	13	52	13	The term "aviation cloudiness" is not very clear. Suggest using "aviation-induced cloudiness" as is done subsequently in the report. U.S. Government
02	052	24		25	The sentence "In reply, Minnis (2005) highlights the uncertainty in evaluating the regional response to regional forcings in GCMs" needs to be expanded to address what these

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					uncertainties are, how valid they are, and how they are related to his conclusions. The IPCC really needs to consider that it is citing a paper that basically said that aviation could solely account for all global warming -- which logically the IPCC does not agree with as evidenced throughout the AR4. So some clear definitive statements re: Minnis' conclusions must be made if these references are maintained. The panel recommends adding a clear, concluding statement to the paragraph. U.S. Government
02	052	44		46	Need to cite evidence of condensed hydrocarbon U.S. Government
02	052	49		51	The study by Hendricks et al. (2005) cited showing the potential for significant cirrus modifications by aviation caused by increased numbers of black carbon particles was based on hypothetical scenarios. This should be acknowledged. U.S. Government
02	056		58		The extended discussion on page 56-58 regarding climate effects of the galactic cosmic ray flux needs a clear statement of what the IPCC assessment is for this contribution. U.S. Government
02	056	38	58	8	Overall, what this section and its conclusion emphasize is the highly uncertain nature of the proposed mechanisms by which indirect solar effects might operate. What ought to be included in the conclusion is an acknowledgement of what is certain: that even if these particular mechanisms are not borne out, it seems clear that that some indirect solar effect on temperature must be at work, given the known strength of the historical correlation between solar activity and temperature. The AR4 should say what mechanisms are certain; and if others are mentioned as possible, this can be accepted. U.S. Government
02	065	26			Confidence levels in RF are cited for all RF mechanisms except aerosols and stratospheric ozone changes. Where do we stand in that front? These are important and need to be addressed.

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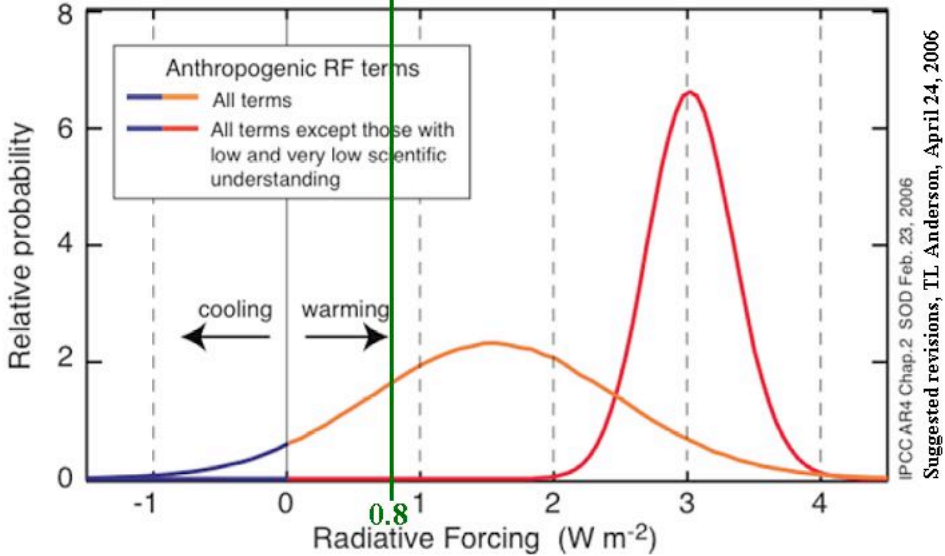
CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
02	065	42	69	42	The IPCC should be commended for its attempt to provide some guidance on comparing the impact of various emissions, and using economic valuation to guide decisions. However, the lack of sector specific information limits utility. We recommend providing such information. U.S. Government
02	067	17		18	The statement, "These summations imply that since 1750, humans have <i>very likely</i> exerted a net warming influence on climate." should be immediately followed by: "An important question is whether the magnitude of anthropogenic RF is large enough to provide a plausible explanation for the observed, industrial-era increase in global-mean surface temperature. Based on a survey of inverse climate-model calculations (Refs 1-6), a minimum threshold of +0.8 W/m ² has been suggested (Ref 7) in order for this causal connection to be legitimate. This threshold is indicated as a vertical line in the lower panel of Figure 2.24. In terms of the well-known RFs associated with greenhouse gases, we see that humans are <i>virtually certain</i> to have exerted a warming influence that exceeds this threshold. When the more uncertain RFs associated with albedo changes are included, the probability distribution expands greatly. Yet even according to this expanded probability distribution, it is <i>likely</i> that the total RF from human activity exceeds the suggested threshold of +0.8 W/m ² ." U.S. Government
02	069	54	70	2	The sentence stating "metric formulation depends" on whether we're interested in the UNFCCC objective or reducing climate change impacts is not clear; what is the distinction? U.S. Government
02	070	04			Define "discounted present value". Reference in Glossary if used elsewhere. Define as used in WG III. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
02	070	09		10	Define “marginal damage costs”. Reference in Glossary if used elsewhere. Define as used in WG III. U.S. Government
02	070	38			Define “time horizon”. Reference in Glossary if used elsewhere. Define as used in WG III. U.S. Government
02	071	14		16	We agree with this statement that a lack of temporal equivalence does not invalidate the GWP concept or provide any guidance to replace it, and suggest it be elevated to the summary of this chapter as a key point. U.S. Government
02	071	24		40	This paragraph on the possibility of developing GWPs for short-lived species needs to be worded much more carefully to note fundamental differences between short-lived and long lived species in calculating GWP. Also, the concept of GWP as derived for radiative forcing is too simplistic to capture the climate effects of absorbing aerosols, including black carbon. Comparing greenhouse gas effect with absorbing aerosol requires the use of climate models that predict the actual impact on surface temperature. U.S. Government
02	128				Table 2.12; the range of values cited for aviation induced cirrus does not appear to be consistent with Table 2.9. This needs to be reconciled. U.S. Government
02	146				Resolution of graphic makes text hard to read U.S. Government
02	158	3		31	We suggest a revision to Figure 2.24, bottom panel, and associated text. This figure shows the probability distribution of total anthropogenic radiative forcing (RF). A rough mock-up of the proposed revision to the lower panel is included below. We strongly support the ideas of discussing total anthropogenic RF and of presenting this as a statistical probability distribution, as is done in the current version of Figure 2.24,

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>bottom panel. This figure includes a vertical line at a total forcing value of zero (W/m²) and the text (page 2-67, lines 17-18) uses this vertical line to conclude that total anthropogenic RF since 1750 is "very likely" to have been positive. This conclusion is deemed important enough to mention in the Chapter 2 Executive Summary (page 2-6, line 19) as well as in the Summary for Policy Makers (page SPM-5, line 1) and in the Technical Summary (page TS-12, lines 19-24).</p> <p>However, the zero line is not the only, and probably not the most important, threshold value to show and discuss. We recommend that a vertical line be added to the lower panel of Figure 2.24 at an RF value of +0.8 W/m². This is the critical value of total anthropogenic RF that emerges from six "inverse" (or "top-down") climate-model calculations (Refs 1-6, below) as summarized by Ref. 7 (below). Values of total anthropogenic RF that are less positive than +0.8 W/m² are inconsistent with every one of these "inverse" studies. Inconsistency, in this sense, means that it would be problematic to posit known RF's as the explanation of the observed, industrial-era increase in surface temperature (about +0.6 K). (Such an inconsistency would imply that the observed warming was caused by currently unknown forcings or that natural variability or climate sensitivity is higher than what climate models currently allow). To show and discuss only the threshold at zero W/m² fails to consider this issue of causal connection and fails to acknowledge and integrate important scientific progress since the TAR.</p> <p>Along with the change to the lower panel of Figure 2.24, the relevant statements in the Chapter 2 text and Executive Summary should be modified to include consideration of whether total anthropogenic RF is a plausible explanation of the observed global-mean temperature increase, as detailed above.</p> <ol style="list-style-type: none"> 1. Wigley, T. M. L., and S. C. B. Raper, Interpretation of high projections for global-mean warming, <i>Science</i>, 293, 451-454, 2001. 2. Harvey, L. D. D., and R. K. Kaufmann, Simultaneously constraining climate sensitivity and aerosol radiative forcing, <i>J. Climate</i>, 15, 2837-2861, 2002. 3. Gregory, J. M., R. J. Stouffer, S. C. B. Raper, P. A. Stott, and N. A. Rayner, An

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>observationally based estimate of climate sensitivity, <i>J. Climate</i>, 15, 3117-3121, 2002.</p> <p>4. Andronova, N. G., and M. E. Schlesinger, Objective estimation of the probability density function for climate sensitivity, <i>J. Geophys. Res.</i>, 106, 22605-22611, 2001.</p> <p>5. Knutti, R., T. F. Stocker, F. Joos, and G.-K. Plattner, Constraints on radiative forcing and future climate change from observations and climate model ensembles, <i>Nature</i>, 416, 719-723, 2002.</p> <p>6a. Forest, C. E., P. H. Stone, A. P. Sokolov, M. R. Allen, and M. D. Webster, Quantifying uncertainties in climate system properties with the use of recent climate observations, <i>Science</i>, 295, 113-117, 2002.</p> <p>6b. Forest, C. E., P. H. Stone, A. P. Sokolov (2006), Estimated PDFs of climate system properties including natural and anthropogenic forcings, <i>Geophys. Res. Lett.</i>, 33, L01705, doi:10.1029/2005GL023977.</p> <p>7. Anderson, T. L., R. J. Charlson, S. E. Schwartz, R. Knutti, O. Boucher, H. Rodhe, and J. Heintzenberg, Climate forcing by aerosols - A hazy picture, <i>Science</i>, 300, 1103-1104, 2003.</p>

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>Is total RF large enough to explain the observed warming?</p> <p>NO ←</p>  <p>U.S. Government</p>
02	158				<p>In Figure 2.24. The 2.24 caption does not state that the uncertainties are one sigma, whereas the other two renditions do note this.</p> <p>U.S. Government</p>
03	00				<p>Throughout the chapter, results of linear trend analyses are presented that include estimates of statistical significance. In two specific sections of the chapter (page 3-9, lines 18-22 and page 3-116, lines 53-56), the comment is made that the statistical significances of trends in</p>

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>variables estimated using Restricted Maximum Likelihood regression (REML) -- which is the method used within the report -- are likely to be overestimated; with citations given for Zheng and Basher, 1999 and Cohn and Lins, 2005. On page 3-116, lines 55-56, after acknowledging that this problem stems from the presence of long-term persistence in the underlying climatic processes, the report then states “Nevertheless, the results depend on the statistical model used, and more complex models are not as transparent and often lack physical realism.” Indeed, the results do depend on the model used and, as pointed out by Cohn and Lins, 2005, simple models (like REML) do not capture the complexity of long-term persistence -- that’s why results based on the use of simple models are in error. The comment that “more complex models are not as transparent and often lack physical realism” contradicts the central point of Cohn and Lins, 2005. If long-term persistence exists within climatic processes, and the 4AR draft says that it does (page 3-116, lines 53-54), then a more complex model, such as that used by Cohn and Lins (2005) MUST be used to estimate statistical significance. This is not a matter of subjective model choice but, rather, of selecting a model that can be demonstrated as capturing the inherent behavior of the process in question. REML, and all other simple linear models, do not capture the observed temporal behavior of land surface temperature, sea surface temperature, precipitation, and any other hydro-climatic variable. The 4AR draft is reporting statistical significances <i>that are known to be gross overestimates</i>. To address this problem, the authors have two choices. One is to recalculate the statistical significance estimates of all variables for which significance is currently reported using a procedure such as Cohn and Lins’ (2006) Adjusted Likelihood Ratio Test that is specifically designed for use with data exhibiting long-term persistence. Alternatively, the report could retain all of the current information regarding trend magnitude (which Cohn and Lins document as being insensitive to the method used to estimate it), but remove all reference to statistical significance -- in text, tables and figures. Indeed, the latter option may be desirable because, as noted by Cohn and Lins, “it may be preferable to acknowledge that the concept of statistical significance is meaningless when discussing poorly understood systems.”</p>

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
03	00				Suggest including more discussion of better characterized embedded shorter period trends to balance discussion of trends computed over long periods. Readers will concentrate on the long-term trends which, when considerable shorter-term variability is present, will be strong functions of the conditions at the start and end of the record and not indicative of important changes on shorter time scales. This comment reflects some of the specific comments received on this chapter concerning the statistical analysis to extract trends from a record containing strong fluctuations at various time scales. U.S. Government
03	00				Use of “likely” and other terms reflecting certainty or confidence of a statement in the chapter are inconsistently applied. There are numerous instances where formal terms of certainty or confidence defined elsewhere in the assessment, in particular, the Technical Summary, have been used to qualify a statement in an informal and inappropriate sense for the assessment. Recommend that the authors conduct a global search and evaluation for consistent use of these terms throughout the volume. These terms include, but are not limited to: “likely”, “caused”, “confidence”, “attribution”. U.S. Government
03	00				Chapter 3 is supposed to focus on results from observations, but frequently went beyond the summary of recent observations in the literature into explanations and discussions of attribution. The discussion on “Mechanisms for longer scale variability” in Section 3.6 seems like a discussion of attribution or speculation, not adequately supported by references. It seems unsuited for the observations section of the assessment. It is more appropriate for Chapter 9 on “understanding and attribution”. These discussions of attribution have extended the length of the observation chapters and lead to an uneven presentation. Strongly recommend removing these discussions, or if appropriate, move them to Chapter 9. Also strongly recommend a substantial shortening of the Chapter 3, 4 and 5 bundle in order to make them more even in presentation, as well as more focused,

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					and improve the ease of reading. U.S. Government
03	00				There are a variety of positions presented in Chapter 3 on some of the large-scale coherent patterns of the atmosphere, such as the AMO discussions. Recommend a thorough review of the use of these terms throughout Chapters 3, 4, and 5 to improve the consistency in the discussion. U.S. Government
03	00				A preponderance of comments received on Chapter 3 was concerned with a general weakness regarding coverage of the water cycle. The authors should evaluate the treatment of hydrology and the water cycle to improve its presentation regarding atmospheric observations. U.S. Government
03	00				This chapter is often quite difficult to read. One of the reasons is that for many of the fields (radiation, clouds, precipitation) the observations are quite equivocal. That in itself would make it difficult, but the presentation does not help. Often paragraphs start off with a definitive statement about the direction of change of a parameter, and then, either in the next paragraph or sometimes even in the same one, conflicting evidence is provided. One has to wait until the summary to disentangle the diverse claims. It would be better if the opening sentence mentioned that there is conflicting evidence for changes, and then, modestly, provide examples of the different results. To a good extent this is what is done in Chapter 8 with the model results, and it helps make that chapter much easier to read. U.S. Government
03	00				The fundamental organization of WG1 and Chapter 3 on observed changes fails to recognize that hydrologic changes are one of the most important geophysical response variables and indicators of climate change. There are chapters on sea-level rise and on snow, ice, and frozen ground but not for hydrologic changes. Timing of Streamflow – Western USA Aguado et al. 1992, J. Climate 5:1468-1483.

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					Timing of Streamflow – NW USA Cayan et al., 2001, Bull. Amer. Met. Soc. 82:399–416.
					Timing of Streamflow - California Dettinger, & Cayan. 1995. J. Climate 8:606-623.
					Timing of Streamflow Dettinger & Diaz J. Hydrometeor. 2000, 1, 289-310.
					Timing of Streamflow - New England Hodgkins et al. 2003 J. Hydrol. 278:242-250.
					Timing of Streamflow – SW Canada Leith & Whitfield. 1998. Can. Water Resour. J. 23:219-230.
					Timing of Streamflow – Lena River, Siberia Yang et al. 2002, J. Geophys. Res., 107(D23), 4694, doi:10.1029/2002JD002542
					Timing of Streamflow – West-Central Canada Burn 1994. J. Hydrol. 160:53–70.
					Timing of Streamflow – Fraser River Canada Morrison et al. (2002) J. Hydrol. 263: 230-244
					Timing of Streamflow NW USA Stewart et al. 2004. Climatic Change 62:227-232
					Timing of Streamflow – Western North America Stewart et al. 2005. J. Climate 18: 1136-1155
					Timing of Streamflow – Hudson Bay Region Gagnon & Gough. 2002. Can. Water Resour. J. 27: 245–262.
					Timing of Streamflow – Eastern USA Czikowsky et al. 2004 J. Hydromet. 5:974-988
					Timing of Streamflow – Mackenzie Basin Aziz and Burn (In Press) J. Hydrol.
					Timing of Streamflow – Liard Basin Burn et al. 2004 Hydrol. Sci. J. 49:69-83
					Timing of Streamflow - Mackenzie Woo & Thorne 2003 Arctic 56:328-340
					Timing of Streamflow – S. British Colombia, Canada Cunderlik, & Burn, 2004. J. Hydrologic Engrg. 9:246-256.
					Timing of Streamflow - Mackenzie Burn et al. 2004, Can. Water Resour. J. 29:283-298
					Timing of Streamflow - Churchill-Nelson Westmacott & Burn, 1997 J. Hydrol. 202, 263-279.

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					Timing of Streamflow – 42 Rivers Central Canada Dery et al. 2005 J. Climate 18: 1540-1557
					Timing of Streamflow NW USA Regonda (2005) J. Clim. 18:372-384
					Decreases in Streamflow Fu et al., InPress, Climatic Change.
					Decreases in Streamflow (Summer) Leith & Whitfield. 1998. Can. Water Resour. J. 23:219-230.
					Decreases in Streamflow (Summer) Prowse & Conly. 1998. Hydrol. Proc. 12:1589-1610.
					Decreases in Streamflow – parts of China Tao et al. 2003 Agricultural For. Met. 118:251-261
					Decreases in Streamflow – Yellow River Jiongxin, X., 2005. Environ. Manage. 35:620 - 631
					Decreases in Streamflow – 42 Rivers Central Canada Dery et al. 2005 J. Climate 18: 1540-1557
					Decreases in Streamflow to Lake Chad (Charli/Logone River Systems) Coe, M.T., and J.A. Foley. 2001. J. Geophys. Res. 106:3349-3356.
					Decrease in Lake Level – Lake Chad Coe, M.T., and J.A. Foley. 2001. J. Geophys. Res. 106:3349-3356.
					Increases in Streamflow - USA Hubbard et al. 1997 Proc. IAHS Publ. No. 226
					Increases in Streamflow - Arctic Lammers et al. 2001 J. Geophys. Res., 106(D4), 3321-3334
					Increases in Streamflow - Global Labat et al. 2004 Adv. In Water Resour. 27: 631-642
					Increases in Streamflow - USA Lins & Slack. 1999. Geophys. Res. Letters

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>26:227-230.</p> <p>Increases in Streamflow - USA McCabe & Wolock 2002. Geophys. Res. Lett. 2002 29(24), 2185, doi:10.1029/2002GL015999</p> <p>Increases in Streamflow - Arctic Peterson et al., 2002. Science 298:2171-2173.</p> <p>Increases in Streamflow – Central USA Mauget 2004 Climatic Change 63:121-144.</p> <p>Increases in Streamflow - USA Groisman et al. 2001. Bull. Amer. Met. Soc. 82:219-246.</p> <p>Increases in Streamflow - Greenland Haq et al. (2002) XXII Nordic Hydrological Conference 2002, NHK/NHC</p> <p>Increases in Streamflow – Major Rivers USA Walter et al. 2004. J. Hydrometeorology 5:404-408</p> <p>Increases in Streamflow - Baspa River Basin, Himalaya Region Kulkarni et al. (2003) Intl. Arch. Photogramm. Remote Sensing Spatial Infor. Sci. 34:1265-1269</p> <p>Increases in Streamflow – Former USSR Georgievsky et al. 1996 Russian Meteorol. Hydrol. 11:66-74</p> <p>Increases in Streamflow - La Plata Basin, South America Berbery et al. (2002) J. Hydrometeorology 3:630-645</p> <p>Increases in Streamflow – parts of China Tao et al. 2003 Agricultural For. Met. 118:251-261</p> <p>Increases in Streamflow – Hudson Bay Gagnon & Gough. 2002.Can. Water Resour. J. 27: 245–262.</p> <p>Increases in Streamflow – Mackenzie R Aziz and Burn (In Press) J. Hydrol.</p> <p>Increases in Streamflow - Sweden Birsan et al. (2005) J. Hydrol. 314: 312–329</p> <p>Increases in Streamflow – South America Garcia & Mechoso. 2006. Hydrol. Sci. J. 50:459-478.</p> <p>U.S. Government</p>
03	003	29		33	The statement that the highest (lowest) 10% of warm (cold) nights has changed is wrong.

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					The percentages are relative numbers and the lowest (highest) 10% are always the lowest (highest) 10%, what has changed are the temperatures of the 10% warmest and coldest nights. Statement in text needs clarification. U.S. Government
03	003	44		50	The TAR concluded that the urban heat island effect could have affected global average surface temperature by as much as 0.12 C. AR4 owes the reader an explanation of why the TAR was wrong, or at the very minimum, an acknowledgement that this finding represents a departure from the TAR. U.S. Government
03	004	48		56	The link between solar intensity and evapotranspiration is not clear, and without this link, the conclusion does not make sense. U.S. Government
03	005	18		19	The AMO is not universally accepted as a true atmospheric circulation pattern. Kerry Emmanuel has given a seminar in which he considers the AMO an artifact of data analysis. He should be contacted to determine if his idea has been published in a refereed journal. U.S. Government
03	005	37		49	Summary here does not quite match table on 3-74 vis-à-vis numbers U.S. Government
03	007	23		27	Suggest providing the information in a table rather than as text. Provide or refer to table in one place in the document, perhaps the Technical Summary table. U.S. Government
03	007	49	8	1	Although the difference between skin and bulk temperature is explained later in the section, this is the first time the two concepts have been given. The difference should be given here. U.S. Government
03	010	13		28	The TAR concluded that the urban heat island effect could have affected global average surface temperature by as much as 0.12°C. AR4 owes the reader an explanation of why the

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					TAR was wrong, or at the very minimum, an acknowledgement that this finding represents a departure from the TAR. U.S. Government
03	011	14			There is considerably more description given to techniques for correcting SST data than to other observations. For example, there is no discussion of the possible effect of the different observing methods (e.g., CTD, bottles, XBTs, etc.) used to generate the World Ocean Data summaries. For parallel discussions, either the SST technique descriptions should be reduced or ideally, the subsurface databases and potential biases between measurement systems should be increased. U.S. Government
03	014	16		23	Why include much discussion of trends computed over long periods when the records are better characterized by the variability of the embedded shorter period trends? Readers will concentrate on the long-term trends which when considerable shorter-term variability is present will be strong functions of the conditions at the start and end of the record and not indicative of the important changes. Thus, the discussion of this type of long-term trends should be limited in the text. U.S. Government
03	015	55			Here you should cite Held and Soden (2000) as a good review of this entire concept of warming effects on the water cycle [Held, I.M., Soden, B, J., 2000. Water vapor feedback and global warming. Annual Review of Energy and the Environment 25, 441-475.] Also in this passage Huntington 2006 should be cited as a review of evidence that is consistent with an ongoing intensification of the hydrologic cycle. Huntington, T. G. 2006, Evidence for intensification of the global water cycle: review and synthesis, Journal of Hydrology, 319:83-95. U.S. Government
03	016	20		23	Why include much discussion of trends computed over long periods when the records are better characterized by the variability of the embedded shorter period trends? Readers will

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					concentrate on the long-term trends which when considerable shorter-term variability is present will be strong functions of the conditions at the start and end of the record and not indicative of the important changes. Thus, the discussion of this type of long-term trends should be limited in the text.) U.S. Government
03	016	20	18	52	The narrative leaves one with a strong sense of inconclusiveness. Suggest adding this paragraph at the end of section 3.3.2.1: <p>“A plausible hypothesis to explain the equivocal trend statistics on global and regional rainfall trends based on a century or less of precipitation data is that the effects of greenhouse gases have not yet risen above the level of natural multidecadal variability having time scales that rival the lengths of the data records. The effects of the Atlantic ocean temperatures on multidecadal Sahel rainfall shifts are well known (Folland, 1986). There are indications for North America that multidecadal variations in precipitation are associated with natural oscillations in Pacific and North Atlantic sea surface temperatures (PDO, AMO, see section 3.6.6) (Enfield et al. 2001; McCabe et al. 2004) and at least one modeling study supports these findings and extends them to western Europe (Sutton and Hodson 2005). Figure 3.3.3 shows that multidecadal swings in precipitation are found at many locations around the world and that regional patterns are incoherently phased, making the identification of a global pattern impossible. Until the effects of greenhouse gases become dominant, or the data records sufficiently long, the identification of secular trends will probably remain uncertain.”</p> U.S. Government
03	020	48		54	Authors should note that the ocean salinity data is consistent with an increase in rainfall over oceans at high latitudes (see Curry, R.G., Dickson, R.R., Yashayaev, I., 2003. A change in the freshwater balance of the Atlantic Ocean over the past four decades. Nature 426, 826-829.) U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
03	021	16		19	This section should cite Walter et al.2004 that convincingly shows that for the conterminous USA that increases in precipitation have been much greater than in runoff – that indicates that evapotranspiration increased quite substantially. Walter, M.T., Wilks, D.S., Parlange, J.-Y., Schneider, R.L., 2004. Increasing evapotranspiration from the conterminous United States. J. Hydrometeorology 5, 405–408. U.S. Government
03	021	21			The evidence for a lengthening of the growing season is consistent with an increasing ET because not only do you need moisture and energy for ET but on vegetated lands you need leaves with open stomates. For most of the northern hemisphere there are consistent reports of lengthening of the growing season by around 2 to 3 weeks in the 20 th century. See for example the following refs. Menzel, A., Fabian, P., 1999. Growing season extended in Europe. Nature 397, 659. White, M.A., Running, S.W., Thornton, P.E., 1999. The impact of growing-season length variability on carbon assimilation and evapotranspiration over 88 years in the eastern US deciduous forest. International Journal of Biometeorology 42, 139 - 145. Schwartz, M.D., Reiter, B.E., 2000. Changes in North American spring. Intl. J. Climatol. 20, 929-932. Wolfe DW, Schwartz MD, Lakso A, Otsuki Y, Pool R, Shaulis N (2005) Climate change and shifts in spring phenology of three horticultural woody perennials in northeastern USA. Internat. J. Biometeor. 49: 303-309. U.S. Government
03	021	47		51	Walter et al 2004 should be cited here for increasing precip, ET and streamflow for the conterminous USA Walter, M.T., Wilks, D.S., Parlange, J.-Y., Schneider, R.L., 2004. Increasing evapotranspiration from the conterminous United States. J. Hydrometeorology 5, 405–408. U.S. Government
03	023	04		17	This paragraph leaves the impression that flooding has increased. Please cite these papers

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>to the contrary; USA (McCabe and Wolock, 2002; Vogel et al., 2002), Canada (Zhang et al., 2001b), Scandinavia (Lindstrom and Bergstrom, 2004; Hyvarinen, 2003), or central Europe (Mudelsee et al., 2003) Kundzewicz, Z.W., D. Graczyk, T. Maurer, I. Piskwar, M. Radziejewski, C. Svensson, and M. Szwed. 2005. Trend detection in river flow series: 1. Annual maximum flow. Hydrol. Sci. J. 50:797-810. Vogel, R., Zafirakou-Koulouris, A., Matalas, N.C., 2002. Frequency of record-breaking floods in the United States. Water Resour. Res. 37, 1723-1731. McCabe, G.J., Wolock, D.M., 2002. A step increase in streamflow in the conterminous United States. Geophys. Res. Lett. 29(24), 2185, doi:10.1029/2002GL015999,2002. 29, 38-1 to 38-4. Lindstrom, G., Bergstrom, S., 2004. Runoff trends in Sweden 1807-2002. Hydrol. Sci. J. 49, 69-83. Hyvarinen, V., 2003. Trends and characteristics of hydrological time series in Finland. Nordic Hydrology 34, 71-90. Zhang, X., Harvey, K.D., Hogg, W.D., Yuzyk, T.R., 2001b. Trends in Canadian stream flow. Wat. Resour. Res. 37, 987-998. Mudelsee, M., Börngen, M., Tetzlaff1, G., Grünewald, U., 2003. No upward trends in the occurrence of extreme floods in central Europe. Nature 425, 166 - 169.</p> <p>U.S. Government</p>
03	023	08			<p>This should include Garcia and Mechoso (2006) for increases in streamflow for all of South America Garcia, N.O., and C.R. Mechoso. 2006. Variability in the discharge of South American rivers and in climate. Hydrological Sciences Journal 50:459-478.</p> <p>U.S. Government</p>
03	023	19		25	<p>This paragraph is incomplete in its treatment of ice break up. In addition to Smith and Zhang you could have cited the following papers that show the geographic extent of these trends:</p>

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>Beltaos, 2002, Hydrol. Proc. 16:789-804 Borshch et al. 2001, Water Resour. 28:194-200 Magnuson et al. 2000, Science 289:1743-1746. Yoo & D’Odorico. 2002. J. Hydrol. 268:100-112. Hodgkins et al. 2005 Climatic Change 71: 319-340 Jasek, M J., 1999. Proc. 14th Intl. Symp. On Ice “Ice in Surface Waters” Kuusisto &. Elo. 2000. Verh. Internat. Verein. Limnol. 27:2761-2764.</p> <p>U.S. Government</p>
03	023	19		25	<p>Include McClelland et al 2006. This paragraph is misleading, it suggests that the same caution should be applied to trends in Eurasian rivers draining to the Arctic Ocean as for agricultural areas in China or other parts of Asia where human influences are extreme. Also the increases in discharge in USA (Walter et al., 2004) and South America (Garcia and Mechosos, 2006) are in areas with major aricultural operations but in spite of this they show increases in discharge.</p> <p>Garcia, N.O., and C.R. Mechoso. 2006. Variability in the discharge of South American rivers and in climate. Hydrological Sciences Journal 50:459-478. Walter, M.T., Wilks, D.S., Parlange, J.-Y., Schneider, R.L., 2004. Increasing evapotranspiration from the conterminous United States. J. Hydrometeorology 5, 405–408. McClelland, J., S.J. Dery, B.J. Peterson, R. Holmes, and E.F. Wood. 2006. A pan-arctic evaluation of changes in river discharge during the latter half of the 20th century. Geophysical Research Letters 33:10.1029/2006GL025753.</p> <p>U.S. Government</p>
03	023	48		53	<p>To be parallel with the paragraphs above it should be noted that SST and teleconnections play a large role in Sahelian Rainfall. (see refs below)</p> <p>Bader, J., and M. Latif, The impact of decadal-scale Indian Ocean sea surface temperature anomalies on Sahelian rainfall and the North Atlantic Oscillation, Geophys. Res. Lett., 30(22), 2169, doi:10.1029/ 2003GL018426, 2003. Giannini, A., Saravanan, R. and Chang, P. 2003. Oceanic forcing of Sahel Rainfall on</p>

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					interannual to interdecadal timescales, <i>Science</i> 302, 1027-1030. Lu, J., and T. Delworth, 2005: Oceanic forcing of late-20 th Century drought in the Sahel/ <i>Geophys. Res. Lett.</i> , submitted. U.S. Government
03	025	1			'This relationship' is opposite what has just been discussed (positive correlation between T and P), [need to clarify time period and areas]. U.S. Government
03	030	40			UAH given for both sides of the argument. U.S. Government
03	030	40			Regarding “Apparent UAH conflict...” rewrite as follows: “In the tropics, the theoretically expected amplification of temperature perturbations with height is borne out by interannual fluctuations (ENSO) in radiosonde, UAH, RSS and model data (Santer et al. 2005) but it is not borne out in the trends of radiosonde records and UAH data.” U.S. Government
03	034	29			Seems odd to be talking about the trend being too large when both before and after this paragraph doubt is indicated concerning the validity of the trend (if the balloon data trend is inconsistent with satellite observations now, what confidence can we have in it for earlier time periods?). U.S. Government
03	034	32		33	The statement “Aviation emits a very small amount of water vapor directly into the stratosphere” needs to be expanded to put in context the direct injection from aviation with other water vapor sources already in that region. U.S. Government
03	036	26			The ISCCP data collection began July 1, 1983, not in June, 1983. U.S. Government
03	036	41		56	Lack of assessment here. Does this problem affect the radiative fluxes or not? First

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					paragraph leads to the impression they don't; second paragraph says it's uncertain. U.S. Government
03	042	14		34	Has activity increased or not? Perhaps an introductory sentence should explain that there is conflicting evidence, before painting the pro and con arguments. U.S. Government
03	043	33		35	Big disagreement in the magnitude of the wintertime stratospheric jet in the extratropics between CIRA and SPARC climatologies - contrary to this sentence. U.S. Government
03	048	44			It appears that the global warming trend over the past century has interacted with the SST signal of EN, giving a perception of stronger and more frequent EN events, which in the equatorial Pacific is the sum of interannual warming due to EN with the global long-term warming trend. Add “The long-term trend in equatorial Pacific SST has contributed to an apparent 30-50% increase in the magnitude of recent El Niño events (Mendelssohn et al. 2005)”. Full citation- Mendelssohn, R., S.J. Bograd, F.B. Schwing, and D.M. Palacios. 2005. Teaching old indices new tricks: a state-space analysis of El Niño related climate indices. Geophys. Res. Lett. 32: L07709, doi:10.1029/2005GL022350. U.S. Government
03	048	50	49	3	Delete “... it is likely ... temperatures.” On Pg, 3-48, lines 51-53. In light of the statement on Pg 3-49, lines 2-3, that determining “...whether observed changes in ENSO are physically linked to global climate change is a research question of great importance.” The statement on Pg 3-48, lines 51-53: “... it is likely that global climate change will interfere and alter El Nino just as El Nino changes the global mean temperature.” Is not justified. Likely is defined as a 66-90% probability of being correct, yet the authors are willing to prejudge the outcome of what they define as a research question of great importance. The text (Pg 3-48, line 51) also states that ENSO is involves heat fluxes of the order of 50 W/m sq. Doubling CO2 concentration involved changing heat flux by only 4.4 W/m sq. The

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					water vapor feedback is estimated to increase this effect by 40-50%. However, these effects are an order of magnitude lower than the effect of ENSO, leaving open the question of whether projected climate change would, in fact, affect the El Nino phase of ENSO. U.S. Government
03	048	50	49	3	The statement on Pg 3-48, lines 51-53: “ ...it is likely that global climate change will interfere and alter El Nino, just as El Nino changes global mean temperature.” Does not seem justified, and should be deleted, in light of the discussion just above indicating that ENSO involves heat fluxes of the order of 50 W/m sq. These heat fluxes are an order of magnitude larger than the projected effects of human activities over the next century. It is far from obvious why the relatively small change in heat flux that is projected to result from human activities should impact on any part of the ENSO cycle. U.S. Government
03	050	15		23	Do these articles demonstrate attribution of the decadal climate change to changes in tropical ENSO evolution, or merely show they coincide? It is equally plausible that mid- and high-latitude changes on decadal scales force the changes in ENSO teleconnections, or they are simultaneously driven by the same variability in forcing. This is quite different from originating in the tropics. U.S. Government
03	052	55	53	21	This discussion of the AMO is excellent but lacks a final paragraph to put it into perspective vis-à-vis greenhouse warming: “The multidecadal oscillations seen in the North Atlantic SST (Figure 3.6.8) mirror very closely the similar variations seen in the average Northern Hemisphere temperatures. This, plus the influence of the AMO on North Pacific temperatures as well as in the North Atlantic (Schlesinger and Ramankutty, 1994; Enfield et al., 2001) suggests strongly that the AMO is a natural influence on global temperatures and that it has alternately obscured and exaggerated generational trends in the warming due to greenhouse gases. Recognition of this is essential as we move out of the current warm

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>phase of the AMO because the tendency of Northern Hemisphere temperatures over the coming decades may once again appear less severe than predicted by models.”</p> <p>In view of the apparent dominance of the AMO in global temperatures, one cannot help but wonder why the AMO has been relegated to the status of “Other Indices” (section 3.6.6) instead of having a section of its own.</p> <p>U.S. Government</p>
03	054	35		37	<p>Do these articles demonstrate attribution of the decadal climate change to changes in tropical ENSO evolution, or merely show they coincide? It is equally plausible that mid- and high-latitude changes on decadal scales force the changes in ENSO teleconnections, or they are simultaneously driven by the same variability in forcing. This is quite different from originating in the tropics. The text here implies that the IPO/PDO changed ENSO behavior after 1976-77.</p> <p>U.S. Government</p>
03	058	27		28	<p>Suggest to modify this sentence and add another, as follows:</p> <p>“Tropical SSTs usually determine where the upward branch of the HC is located. However, during the transition from boreal summer to winter, the heating source for the HC in the Western Hemisphere shifts from the Western Hemisphere warm pool centered near the Caribbean Sea (Wang and Enfield, 2003) to the Amazon region in northern South America (Chelliah and Bell, 2004).”</p> <p><u>Reference:</u> Wang, C., and D.B. Enfield, 2003: A Further Study of the Tropical Western Hemisphere Warm Pool. <i>J. Climate</i>. 16(10), 1476-1493.</p> <p>U.S. Government</p>
03	058	38		40	<p>Add two more sentences:</p> <p>“It is also possible to interpret the recent upward trend in HC strength to a natural multidecadal oscillation having a time scale that transcends the length of reliable sounding records (Chelliah and Bell, 2004). Until the issues of data integrity and natural variability are satisfactorily resolved, we can conclude little regarding the</p>

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					possible relationship of the HC trend to greenhouse warming.” U.S. Government
03	065	42			Seems odd to say that the figure is not shown because it is not reliable [nor publication referenced], yet then discuss it for several more sentences. Why should we conclude that the discussion is reliable? U.S. Government
03	066	41		42	There is no basis for saying that “ <u>most</u> of the present warming is associated with global SST increases rather than the AMO”. Suggest the following modification of this sentence: “Nevertheless, it appears that a significant though lesser portion of the present warming is associated with global SST increases rather than the AMO.” U.S. Government
03	067	28			How is the eastern North Pacific affected by the AMO? U.S. Government
03	072	10			Katrina was the most damaging storm on record, but this statement is incomplete. Most of the damage was the result of the failure of New Orleans’s flood control system, a failure which could have been avoided had the system’s documented shortcomings been repaired. The discussion of Katrina’s impacts and how they might have been avoided belongs in WGII’s report. WGI should limit itself to a discussion of the storm’s characteristics, and the degree to which they are related to recent changes in climate. U.S. Government
03	072	18		22	Could note that the vertical wind shear was not particularly favorable. U.S. Government
03	137				Figure is impossibly small to read U.S. Government
04	00				There should be an emphasis on temperatures and links to Chapter 3 discussions, just as there is for accumulation rates. This interannual variability explains why the trends

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					considered over different intervals are different. Seasonality of the trends are another important factor in apparent differences between studies – for example, the Thompson and Solomon (2002) cooling is limited to summer and autumn, and is not an annual mean trend. The current text overlooks these factors and implies that all the trends quoted are annual mean, which is incorrect. There is further confusion in the summary of the new Chapman and Walsh study. U.S. Government
04	00				A possible deficiency of this chapter is that it evidently fails to present the information on Antarctica (and perhaps Greenland) in the format required to ensure that the Chapter 10 authors use its results. Surely the Chapter 4 authors recognize the disconnect between this chapter’s finding that the major ice sheets are <u>already</u> making a net contribution, and the Chapter 10’s finding that they will not do so until <u>after the year 2100</u> . The discrepancy may result from different schools of thought being represented in the different chapters, but the authors of Chapter 4 should re-examine their chapter to evaluate whether an alternative specification of their own findings might make it easier for the results to feed into Chapter 10. For example, what is the uncertainty range for the historical sensitivity of Antarctic ice sheet (mm/yr) to temperature changes? Such a parameterization could be passed directly to the Chapter 10 assessment. Chapter 4’s failure to provide a climate sensitivity parameter leaves Chapter 10 with little choice other than to use pre-existing models, which may or may not include all of the insights embodied in Chapter 4. U.S. Government
04	00				Suggest including more discussion of better characterized embedded shorter period trends to balance discussion of trends computed over long periods. Readers will concentrate on the long-term trends which, when considerable shorter-term variability is present, will be strong functions of the conditions at the start and end of the record and not indicative of important changes on shorter time scales. This comment reflects some of the specific comments received on this chapter concerning the statistical analysis to extract trends from a record containing strong fluctuations at various time scales.

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
04	00				Use of “likely” and other terms reflecting certainty or confidence of a statement in the chapter are inconsistently applied. There are numerous instances where formal terms of certainty or confidence defined elsewhere in the assessment, in particular, the Technical Summary, have been used to qualify a statement in an informal and inappropriate sense for the assessment. Recommend that the authors conduct a global search and evaluation for consistent use of these terms throughout the volume. These terms include, but are not limited to: “likely”, “caused”, “confidence”, “attribution”. U.S. Government
04	00				Chapters 4 is supposed to focus on results from observations, but frequently went beyond the summary of recent observations in the literature into explanations and discussions of attribution. For instance, the section on “Consequences” in Chapter 4 seems wholly out of place. Such discussions of attribution have extended the length of the observation chapters and lead to an uneven presentation. Recommend removing these discussions, or if appropriate, moving them to Chapter 9. Also strongly recommend a substantial shortening of Chapters 3, 4, and 5 in order to make them more even in presentation, as well as more focused, and improve the ease of reading. U.S. Government
04	00				Was there a specific decision made to limit discussion of NAM/SAM influence on sea ice and Arctic/Antarctic temperature change to other chapters? U.S. Government
04	00				Provide observation-based support (or explain the lack of support) for a paragraph in Chapter 10 (page 60, especially lines 20-23). The statement says, in effect, that the major ice shelves cannot have significant surface melt unless the average summer temperature is above freezing. Given the importance of surface melting to Chapter 10’s analysis, it would be helpful to discuss (a) whether periods less than an entire summer can induce significant melting and (b) whether interannual variability is enough for significant melting to occur

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					during some years while the mean summer average remains below freezing. U.S. Government
04	002	16			Of course this relationship works both ways - with reduced snow cover, there will likely be warmer temperatures, both from the standpoint of albedo change as well as lack of melting that keeps temperatures nearer to 0°C. These points are noted later, but some comment about the interactive nature of the feedback may be appropriate here. U.S. Government
04	002	42	2	50	Please reconcile the apparent current positive contribution from Antarctica with the projected negative contribution to sea level rise from Antarctica in Chapter 10. U.S. Government
04	004	25			Replace "frozen ground" with "permafrost" as this is the relevant long-term condition. Chapter needs to review and be consistent about definition of frozen ground. U.S. Government
04	005	44			The statement that frozen ground is the most vulnerable based simply on extent is debatable. I would think snow cover and sea ice cover are shown to be more susceptible to change. There is no criteria provided for vulnerability. Frozen ground is more resistant to change than snow. I would delete this conclusion here and in all other places. The ACIA report does not make this conclusion. U.S. Government
04	005	45		48	The text says, "Frozen ground can translate climatic change to other environmental components and facilitate further climate change through the impacts on greenhouse gas exchange between the atmosphere and the land surface." The meaning of this is not clear. U.S. Government
04	005	47			Remove "impacts on" U.S. Government
04	009	35			Repeated phrase "...of the country". U.S. Government

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04	012	4		32	The actual words such as ice draft and freeboard should be defined or referred to the glossary the first time they are measured. U.S. Government
04	014	19			Incomplete sentence. U.S. Government
04	018	26		28	What are the implications of this assumption and inference? U.S. Government
04	019	2			Is this contribution negative? And use common units for mm per year. International System of Units. U.S. Government
04	019	43			Odd way of saying this...not sure what it means. U.S. Government
04	020		24		Somewhere in this section—probably at the beginning or end—you need a paragraph that pulls things together as well as lines 42-50 on page 2. SPM ought to quote lines 42-43 on page 2, but it’s style is to reference specific sections in the chapters. U.S. Government
04	025	23		24	Change sentence on line 23-24 to, “Studies of Antarctic surface temperatures similarly show strong interannual variability linked to the major modes of Southern Hemisphere atmospheric circulation (Schneider et al, 2004).” Note that Schneider et al (2004) was not a trends study as implied in the draft text. U.S. Government
04	025	25		25	Sentence should start, “A recent trend analysis of temperature data poleward of 50 deg S from 1958-2002 shows overall annual mean warming for land. Ocean and the whole domain over that interval (Chapman and Walsh, in press).” Don’t use “reanalysis” because it can be confused with the meteorological reanalysis, e.g. ERA-40, NCEP. U.S. Government
04	025	28			Delete “and”; Start a new sentence with “These results [REFERRING TO CHAPMAN

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					AND WALSH] are consistent with other recent studies, showing that the strongest trends are wintertime warming over the Antarctic Peninsula (ref. suggested: Turner, 2005) and the summer and autumn cooling over other areas of the continent (refs Doran et al, 2002; Thompson and Solomon, 2002).” U.S. Government
04	025	28		30	Sentence starting with “Furthermore” is not very clear as to location or meaning of “full interval.” The Chapman and Walsh study shows these claims are true for land only. Change sentence to: “Furthermore, annual mean trends on the Antarctic continent are dependent on the intervals considered, with trend analyses similarly ending in 2002, but starting between 1966-1982 showing cooling, and starting between 1958-1965 showing warming.” U.S. Government
04	025	30			Delete the sentence starting with “Thus,.” Now the cooling is covered with the sentence above. Suggested conclusion: “Thus, while the full 1958-2002 interval shows warming over Antarctica, it is important to consider the seasonality of the changes and the strong interannual variability when making interpretations.” U.S. Government
04	028	48			Delete phrase about vulnerable because vulnerable not a defined term U.S. Government
04	029	18			Add new sentence “Romanovsky et al (2002) summarized results of many of these recent measurements. (add the following reference: Romanovsky, V. E., Burgess, M., Smith, S, Yoshikawa, K., and Brown, J., 2002. Permafrost temperature records: Indicator of climate change. Eos 83 (no. 50), pp.589, 593-594.” U.S. Government
04	029	22			Reword. Temperatures at the top of the permafrost U.S. Government
04	030	2			Replace “powerful” with “more direct”

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
04	030	3			Add after Murtel-Corvatsch “in the Swiss Alps” U.S. Government
04	030	20			This table should be cross-referenced to Romanovsky et al 2002 and the updated version on page 211 of the ACIA report. The non-permafrost reader will be unaware that long-term changes are based on an extrapolated curve to the top of the permafrost table, thus the approximate 1 meter depth (base of active layer) for Lachenbruch and Marshall. Author should try to make this distinction otherwise the temperature changes are like comparing apples and oranges. Furthermore the period of record is not 1910s as there were no observations at that time in Northern Alaska; table is incorrect. Northern Quebec entry for 1996-2001 cites a 1995 reference, Change to Brown et al 2002 or Smith. 2005. Russia it’s 2001 not 1002. U.S. Government
04	031	4		47	This section would benefit from more integration with Chapter 6 of the ACIA report (pages 209-220). A key recent reference on degradation is the paper by Jorgenson on the Tanana Flats. Lines 43 -45: speculative sentence, recommend deletion, not related to observations. U.S. Government
04	031	52		53	Subsea permafrost did not form as a result of inundation. It formed when continental shelf was exposed to colder climates at low stands in sea level. U.S. Government
04	032	19		25	Difficult to follow which stations cited in text are being used for the results shown in Fig 4.7.1. Pavlov and Malkova have a 2005 publication that re-states many of these onclusions. Every effort should be made to update this Russia section based on the 2005 report. U.S. Government
04	032	20			Early 1990s. the original 66 permafrost stations had been reduce to 25

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
04	032	29		41	Spell out GTN-P. This section could be updated using papers published in special issue of Permafrost and Periglacial Processes (see Nelson 2005) U.S. Government
04	032	48			underlying instead of underlain U.S. Government
04	032	49		51	Need to specify if these stations are in previously forested areas and were cleared and now covered by grass. Do they represent regional vegetation. U.S. Government
04	032	53			Are these soils on the Plateau underlain by permafrost. It's a permafrost region? U.S. Government
04	033	4		8	Rewrite first sentence; poorly phrased. Would be good to explain how these changes were computed; presumably not measured?? U.S. Government
04	033	22		52	Presumably the thaw would also have some impact on CO ₂ uptake during the growing season. Why are consequences discussed in a chapter on observations? Shouldn't consequences of observations appear elsewhere in the Assessment? U.S. Government
04	033	36			Why would these changes have resulted in increased runoff? One would have thought with more infiltration, to greater depth, at least surface runoff would have been reduced. U.S. Government
04	034				Near Table 4.8.1, it would be valuable to add another table listing the potential sea-level change (total stored freshwater) that could result from total melting of each of the components listed in 4.8.1. U.S. Government
04	034				Please discuss Chapter 10, which seems to be quite confident that Antarctica will have a negative contribution. That seems to contradict the positive Antarctic contribution that this

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					chapter finds. Are there two schools of thought that each need to be reflected in both chapters? U.S. Government
04	053				Figure 4.3.2 is hard to see, message is difficult to grasp. Graphic artist should redo. U.S. Government
04	055		56		Figures 4.4.1 and 4.4.2 show the evaluation of only a single data set on sea ice extent and concentration and no error bars for the individual estimates for each year. There are a number of estimates of sea ice extent and concentration using essentially the same data sets by very different algorithms. These estimates can differ by more than 30%, especially for new ice or melting ice, which are critical to these estimates. Please provide a true assessment of the variations in sea extent and concentration from using the published estimates for these values from various authors. These curves are sure to be quoted in discussions about the outcomes from AR4 and it is not fair to only provide the assessment of this change from only one estimate. U.S. Government
04	066				Figure resolution is poor, and caption needs to stand alone and be clear. U.S. Government
04	071				Figure 4.8.1 is very important summary figure, but it has no punch, no impact. Needs to be more attractive, eye-catching, as it will likely be grabbed and used by many speakers and educators. U.S. Government
04	71				Prefer use of permafrost in diagram (below word snow and in caption) since bullet refers to permafrost temperature. U.S. Government
05	00				Suggest including more discussion of better characterized embedded shorter period trends to balance discussion of trends computed over long periods. Readers will concentrate on

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					the long-term trends which, when considerable shorter-term variability is present, will be strong functions of the conditions at the start and end of the record and not indicative of important changes on shorter time scales. This comment reflects some of the specific comments received on this chapter concerning the statistical analysis to extract trends from a record containing strong fluctuations at various time scales. U.S. Government
05	00				Use of “likely” and other terms reflecting certainty or confidence of a statement in the chapter are inconsistently applied. There are numerous instances where formal terms of certainty or confidence defined elsewhere in the assessment, in particular, the Technical Summary, have been used to qualify a statement in an informal and inappropriate sense for the assessment. Recommend that the authors conduct a global search and evaluation for consistent use of these terms throughout the volume. These terms include, but are not limited to: “likely”, “caused”, “confidence”, “attribution”. U.S. Government
05	00				Chapter 5 is supposed to focus on results from observations, but frequently went beyond the summary of recent observations in the literature into explanations and discussions of attribution. Strongly recommend removing these discussions, or if appropriate, moving them to Chapter 9. Also strongly recommend a substantial shortening of the Chapter 3, 4, and 5 bundle in order to make them more even in presentation, as well as more focused, and improve the ease of reading. U.S. Government
05	00				Recommend a thorough review of the terminology associated with large-scale coherent patterns of the atmosphere (such as AMO) throughout Chapters 3, 4, and 5 to improve the consistency in the discussion. U.S. Government
05	002	53			Point 11 should give the contribution of loss of mass from glaciers, ice caps, etc. for the last 50 years for the point made on the next page (3:1) to be illustrated more clearly. For

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					the last 10 years, the differences between the observed value (3.1) and the two components (2.8) are not large. U.S. Government
05	003	04		6	The fact that sea level is not uniform is not intuitively obvious. Adding the statement from Pg 5-35, lines 1-2 "Spatial variability of sea level (rise) rates is mostly due to non-uniform thermal expansion." would provide an answer to the obvious question in many readers' minds. U.S. Government
05	003	12		15	Why can't changes in heat content and salinity, for example, described as consistent with known characteristics of ocean circulation also be consistent with characteristics of surface energy fluxes as well? U.S. Government
05	005	7		28	In sentence 5 it is stated that results for this section are based on WOD2001, yet in sentence 28 results to 2003 are given. This implies another database was used and this should be explained. U.S. Government
05	005	28		30	Are the reported trends, the rate of heat content change? If so, the units should include time (-1). U.S. Government
05	005	34			The reader is referred to the Appendix to get a sense of the time-dependent biases that may affect the changes and variations of the ocean heat content data sets. However in the Appendix no assessment of these biases is provided. Rather the reader is told that the similarity between the large less well-calibrated data used to derive ocean heat content changes and the specific research voyage data gives credibility to the ocean heat content data sets. Yet the reference quality data sets in Section 5.3 focus on circulation and water masses, not heat content. It would seem appropriate to discuss any general assessment of time-dependent biases, or comparison of independent observing systems, as has been done

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					with other data sets, e.g., tropospheric temperature, precipitation. Given that the community has not been able to do this yet, the authors should be more explicit about the lack of our ability to ascribe errors in the trends due to time dependent biases due to changes on observing methods (as opposed to analysis methods). U.S. Government
05	005	35			Can we really 'note' this if it is the first time it is being presented to us? Drop the "note that" phrase. U.S. Government
05	005	43		49	This paragraph cites a correlation as evidence for validity of the subsurface ocean data, but surely there must be some lag in the transport of heat, and this is never discussed or shown. It is of most interest to show the low frequency relationship, not the high frequency as related to the trends discussed in Chapter 5 U.S. Government
05	005	43		44	Why aren't SST data used in heat content estimates? An explanation is needed. U.S. Government
05	006	6		7	There are trends in Gulf Stream position as shown by Joyce et al. and Molinari which cause large changes in heat content in the region of the separated boundary current. The trends in North Atlantic heat content could be related to these shifts and this should be indicated in the text. U.S. Government
05	006	49		50	Why present the later time period first in each case, if the text says 'first warmed then cooled'? U.S. Government
05	008	31		33	Can one really have high levels of confidence for a value that can't be directly observed and is the net result of a sum of other positive and negative numbers? U.S. Government
05	008	50			Can't really have less agreement when the previous number cited came from only one

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					calculation. U.S. Government
05	010	8			Is there a specific reason why low volumes are associated with warmer water? Is it due to lack of vertical mixing? Some explanation should be stated, perhaps just a few words in the sentence. U.S. Government
05	010	19		22	The NAO and Gulf Stream transport are strongly anti-correlated. Bryden et al. only found strengthening of the eastern limb of the gyre (i.e., the Florida Current transport has stayed relatively stable over the past 40 years), a weakness in their argument for gyre changes that should be noted. U.S. Government
05	011	9			Which decades did this occur in? U.S. Government
05	011	34			Change slowdown to decrease. U.S. Government
05	011	46			Repeats the statement from page 11, line 20. U.S. Government
05	012	7		14	The apparent inconsistency between a decrease in Nordic Sea subsurface salinity and increase in inflow salinity from the Atlantic needs to be explained. U.S. Government
05	012	20		24	Was this 'redirection' purposeful, an anthropogenic effect that changed the convection noticeably in the Arctic? Is it known why the water subsequently freshened? U.S. Government
05	012	50		51	All the dense waters of the MOC doesn't sink to the seafloor (e.g., Labrador Sea Water); sentence should be corrected. U.S. Government
05	013	43		47	Remove “structural” and just cite as “uncertainties”.

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
05	014	17		18	The long-term heat content trend includes the positive PDO state, rather than saying it is related to the PDO. U.S. Government
05	015	4		7	Does the warming at the leading to cooling in the sw Pacific arise because the source has weakened? U.S. Government
05	019	32			Not a sentence... U.S. Government
05	019	53	20	43	The discussion of the methodology for estimating past rates of ocean carbon uptake needs to be expanded to give some indication of the magnitude of corrections to observed dissolved inorganic carbon, and how these corrections are made. Changes in the rate of ocean carbon uptake are a critical factor in projecting climate change, and readers should be able to judge for themselves the validity of these estimates. U.S. Government
05	020	3			If mapping errors do not include uneven data distribution then the latter should be included as a source of uncertainty (particularly data voids). Please describe what is included in mapping errors. U.S. Government
05	021	25			Can the shoaling of the saturation horizons be related to vertical movement of density surfaces? If so, this cause of changes in horizon depths should be given. U.S. Government
05	023	22			If ocean circulation, 'what' persists into the future? U.S. Government
05	023	25		27	Why do circulation changes affect oxygen more than temperature? Changes in advection due to circulation variability could affect oxygen more but because of geostrophy it is not clear that temperature is changed more by gyre changes for example. Fact needs to be

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					verified. U.S. Government
05	023	39			As stated above, a box indicating the significance of the small sea level changes to features that can be comprehended such as the rate of flooding of specific coastal areas or islands is needed to put these numbers in perspective. U.S. Government
05	029	50			Sentence needs editing, add verb. U.S. Government
05	029	51			Is it Levitus or Antonov (see line 42) who made the estimate for 1993-2003? U.S. Government
05	030	48	31	1	Changes in the ocean's thermal structure are driven by surface heating effects not only ocean circulation as is implied in this sentence. U.S. Government
05	032	8			Here and in section 5.5.3, it is stated that only 25% of global sea level rise is due to thermal expansion. This does not seem to agree with Table 5.5.2. U.S. Government
05	032	19		21	Why isn't increased precipitation given as a potential cause of decreases in ocean salinity? U.S. Government
05	034	40		42	Are the higher rates obtained by satellites also seen by local measurements? If not, the higher recent rates would be due to the change in observational technique. Omit phrase in parentheses. U.S. Government
05	035	1			Insert the word "change" between "sea level" and "rates." The current wording is unclear. U.S. Government
05	036	15			Except the western tropical Pacific has shown quite strong positive temperature anomalies since 2000, so much so that it has been blamed for the pattern of droughts that have been seen in NH mid-latitudes. Perhaps this perspective should be mentioned to bring the point

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					up to date. U.S. Government
06	00				In the case of Chapter 6, the Executive Summary is in the format of five questions. For the Chapter 8 question, there is also a summary paragraph at the end of the reply. In the case of several of the boxes in Chapter 3, there are also summaries. An inconsistent structure conveys a message of lack of coordination between chapters. Secondly, the approach of highlighting key findings in the chapeau provides important points to readers that may be skimming the chapter for salient points. Recommend that the Executive summary of all chapters follow a consistent structure. Chapter 3 serves as a good example to follow. U.S. Government
06	00				Throughout Chapter 6, the authors need to make sure to be absolutely clear whether past climatic conditions cited in the text originate from proxy data, spatial reconstructions, or paleoclimatic models. This is currently unclear in many parts of the chapter, leading readers to believe that modeled temperatures are based directly on proxy data and vice versa. U.S. Government
06	00				It is clear in reading this chapter that it was written by multiple authors exercising varying degrees of scientific rigor. The coordinating lead authors need to exercise a stronger role in implementing consistency in both the writing and the scientific integrity of the chapter. For example, if there is not sufficient data to conduct attribution studies of Southern Hemisphere warming over the last 700 years (page 34, lines 48-50), how can page 23, lines 43-48, compare global reconstructions with the late 20th century? Also, the coordinating lead authors need to make sure that the SPM is completely consistent with Chapter 6. U.S. Government
06	00				This chapter will go a long way to integrate paleoclimatic data into the climate change debate. However, to inform policymakers, this chapter must reveal the limitations on how

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					well we can truly identify the leads, lags, contemporaneous relations, and rates of change recorded by disparate paleoclimatic proxy records. Dating uncertainties and temporal resolution influence our ability to develop the coherent paleoclimatic reconstructions used to identify the physical mechanisms for the observed changes. To be fully transparent, this chapter must identify the limitations as well as the findings. U.S. Government
06	00				In general the use of tree rings for climate reconstruction is problematic for reasons that are not addressed in the report. There are strong probabilistic relationships between paleoclimatic records, including tree-rings, and climate. Because of this relationship, tree rings provide one of the strongest paleoclimatic proxy records when given through appropriate statistical treatment. This should be addressed in the final paragraph of Section 6.2.1.4. All paleoclimatic proxy methods have limitations and these limitations need to be adequately addressed in Section 6.2.1.4. Chapter 6 needs to provide an explicit explanation of what we know, how well we know it, and what we cannot know through paleoclimatic records. U.S. Government
06	00				The authors of the chapter have done a great job in providing a balanced and concise assessment of paleoclimate information relevant to climate change policy. Add a brief discussion or mention of paleo records for interactions and feedbacks between deglacial and Holocene climate change and terrestrial carbon cycle. Increasing evidence from peatlands suggests that peat carbon store and accumulation rates have responded to climate variations and, as a result, contributed to atmospheric CH ₄ and CO ₂ budget during the last 15,000 years. Potential additions could go in Section 6.4.2.1 (p. 18) and 6.5.1.2 (p. 22). Suggested references include: Smith et al. 2004. Science 303: 353-356; Yu et al. 2003; Vitt et al. 2000. Can. J. Earth Sci. 37: 683-693. The Holocene 13: 801-803. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
06	00				My comments on the paragraph summarizing the contribution of McIntyre and McKittrick constitute one methodological review by climate science outsiders, qualified in the underlying statistics, who apply modern business standards. Secondly these are largely the efforts of one man over a relatively short time, have been a factor (though not the only factor) in a substantial upward revision in the stated warmth of the MWP since the last TAR, and are ongoing. Therefore it would be wise to assume that the sources of uncertainty identified are important, not complete, and not restricted to this part of the chapter, or to the report overall. While this may seem like drawing an unnecessarily large circumference around a problematic area, it is consistent with a trend in other parts of the report towards more conservative estimates of the magnitude of climate change than were described in the TAR, which my time limitations prevent me from detailing U.S. Government
06	002				All key finding bullets should have levels of certainty attached. For example, the second (page 2, lines 13-16) and fifth (page 2, lines 26-29) are stated as truisms. U.S. Government
06	002	31		33	This bullet is unclear. Change bullet to read: "Using estimated radiative forcing and land surface changes of the Last Glacial Maximum, climate models can simulate many of the broad-scale patterns of climate change reconstructed from paleoclimatic data." U.S. Government
06	002	43		47	Attribution of abrupt climate change only to changes in Atlantic Ocean circulation ignores other explanations including possible nonlinear responses of tropical Pacific variability to radiative forcing directly overhead (Clement et al. 1997; Cane and Clement 1999; Mann et al. 2005). These two leading theories may be partly reconciled by emerging evidence that big changes in the Atlantic can modulate ENSO frequencies, (see recent paper by Dong et al. 2006. <i>Geophysical Research Letters</i>), possibly at multiple time scales. Note that allusion is already made to the dynamic ocean thermostat theory on another of the major findings (page 6-3, lines 39-41).

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
06	003	10		16	<p>This finding is a bit overstated given the data limitations. The time scales of all of the Holocene warming events cited here are different than the time scale of the late 20th century warming. There is a lack of interannual resolution at global coverage for practically all of these events. If in hand, comparable warming over a few decades could be discerned at other times in the Holocene. This mismatch in temporal scales and global coverage in the comparison of warm spells in the Holocene with late 19th century warming needs to be addressed in Section 6.5.1.3. Change sentence starting in line 14 to read: "However, data coverage, temporal resolution, and age control of available proxy data make it impossible to discern if the earlier Holocene contained 50 year periods of global warmth comparable to the late 20th century."</p> <p>U.S. Government</p>
06	004	23		30	<p>Insert "Based on proxy data from 26 locations," before the sentence that starts "It is also.." Figure 6-11(a) shows that the conclusion that it is likely that the second half of the 20th century was the warmest period in the NH in past 1000 years is based on extremely limited information, with proxy data from only 26 sites. This information should be included with the conclusion as an indication of its basis. Limited geographic coverage of proxy sites is noted in the text (pages 29, 32) and needs to be noted in the executive summary. Authors should double check the number of sites and include the tally.</p> <p>Suggest text in page 4, lines 29-30, be changed to read "These conclusions are most robust for summer in extra-tropical land areas and for more recent periods because of the uneven spatial and temporal coverage, and varied characteristics, of the different proxy data."</p> <p>For the sentence starting on page 4, line 26, change to read: "It is also likely that in the Northern Hemisphere this was the warmest 50-year period in the past 1000 years and the warmest 100-year period in the past 1300 years." And delete the following sentence "The regional extent ... during the last 1300 years".</p> <p>U.S. Government</p>

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
06	006	23		24	It might be worth adding the reason that for the last 2000 years being relevant to policymaking here. Delete sentences from 21-24 “We also ... policy making.” And replace with “Much of the chapter focuses on the last 2000 years because of the quality and quantity of high-resolution proxy records and similarity to modern boundary conditions makes this period most relevant to climate change policy and decisionmaking.” U.S. Government
06	006	24		25	This sentence should be moved to page 6, line 6. U.S. Government
06	007	14			Insert text to read “time control gets weaker farther back in time, making it difficult to address issues of leads, lags, and synchronicity that are critical to evaluate and understand climate processes.” U.S. Government
06	007	44			Should read “harvest data, for reconstructions of past climate.” Reviewer 48 U.S. Government
06	007	50		51	“Networks of tree-ring width and tree-ring density are used to infer past temperature changes...” Not only temperature, but moisture-related variables as well! U.S. Government
06	007	53			Should read “functions that are calibrated” U.S. Government
06	008	44			“Greenhouse gas concentrations are not an <i>external</i> forcing, but internal to the climate system.” Delete sentence in 44-46 and end sentence in 43-44 by adding “using differences in proscribed forcing and configuration of oceans and continents.” U.S. Government
06	009	22			The stomatal index has periods for which population-level data of extant species is lacking. The empirical relations between stomatal index and atmospheric CO ₂ concentrations are based on modern species and lots of measurements. Stomatal index tends to vary

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					dramatically within an individual plant and across plants in the same populations. CO ₂ reconstructions based on a few leaves from an extinct species preserved in a few layers of sediments over millions of years is suspect at best. A complicating factor is that temperature and relative humidity cannot be held constant; these factors also affect stomatal densities (see one exception where attempt was made to hold constant in Van de Water, P.D., Leavitt, S.L., and Betancourt, J.L. 1994, Trends in stomatal density and ¹³ C/ ¹² C ratios of <i>Pinus flexilis</i> needles during last glacial/interglacial cycle. <i>Science</i> 264, 239-243). U.S. Government
06	009	26			Should read, “(e.g., emissions associated with periods of more intense volcanic activity and CO ₂ drawdown associated with silicate mineral weathering during major episodes of mountain building).” U.S. Government
06	009	53		54	Delete “Temperature reconstructions for this time period from”. Should now read, “Both terrestrial and marine paleoclimate proxies (Thompson, 1991; Dowsett et al. 1996; Thompson and Fleming, 1996) show that high latitudes were....” U.S. Government
06	010	14			Should read, “tropical temperature change without strong increases in ocean heat transport (Rind and Chandler, 1991).” U.S. Government
06	010	54			Delete “excellent” U.S. Government
06	011	38		40	Should read, “For example, the CO ₂ increase from ~185 ppm at the Last Glacial Maximum to ~265 ppm in the early Holocene occurred in distinct phases (Stennie et al. 2001) (see Figure 6.4).” U.S. Government
06	011	47			Should read, “Within the past 200 years,...” U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
06	012	01			Should read, “the average rate of increase....” U.S. Government
06	012	11			“Change ‘0 to 1800 AD’ to “1 to 1800 AD’ – there is no such thing as 0 AD” U.S. Government
06	012	19			Language found in the Technical Summary (page 13, lines 10-13) should be inserted here. U.S. Government
06	012	32			Should read, “There is no consensus, however, about the exact cause and nature of these ocean circulation changes.” U.S. Government
06	012	48			There should be a period after “biogeochemical feedbacks.” U.S. Government
06	016	31	16	32	Should read, Bond et al. (2003) U.S. Government
06	017	56			Should read, “There is no evidence of mechanisms that could mitigate....” U.S. Government
06	018	34			Section 6.4.2.1 may be the place to briefly discuss contributions (to atmospheric CH ₄ and CO ₂) from and climate responses of northern (boreal and subarctic) peatlands. Suggested references include: Smith et al. 2004. Science 303: 353-356; Yu et al. 2003; Vitt et al. 2000. Can. J. Earth Sci. 37: 683-693. The Holocene 13: 801-803. U.S. Government
06	018	36			Should read “appears centered” U.S. Government
06	019	33		35	The so-called dynamic ocean thermostat model espoused by Clement et al. 1996 and Cane and Clement, 1999, and others needs more explanation. How about, "Some authors have argued that some of the abrupt climate shifts discussed could have been triggered from the tropics. Based on modeling and supported to some extent by compelling evidence of

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>abrupt climate change in the Pacific sector, Clement and Cane (1999) argue for a dynamic ocean thermostat, whereby seasonal insolation maxima and direct radiative heating of the tropical Pacific actually increases upwelling and cooling of the east equatorial Pacific. This reinforces a steepened east-west sea surface temperature gradient and a semi-permanent La Niña-like state with global teleconnections consistent with much of the global evidence for abrupt climate change. This dynamic ocean thermostat model has been invoked to explain the climate of the early-mid Pliocene (Rickaby and Holleran 2005), the early Holocene (Clement and Cane, 1999), and the last 1000 years (Mann et al., 2005).</p> <p>Rickaby, R.E.M. and Holleran, P. 2005. Cool La Niña During the Warmth of the Pliocene? <i>Science</i> 307, 1948 - 1952.</p> <p>An abridged version of the above would also be acceptable.</p> <p>U.S. Government</p>
06	020	16			<p>Should read, “although the trigger for the ocean circulation changes remains undetermined.”</p> <p>U.S. Government</p>
06	20	38			<p>Section 6.4.3 needs an introduction that simply defines the salient issues to be addressed. This should be cross-referenced to sea level discussions in Chapter 5.</p> <p>U.S. Government</p>
06	20	48		51	<p>Statement is cryptic and potentially incorrect; needs to be rewritten. This should be cross-referenced to quantification of TOPEX/Poseidon corrections in Chapter 5 as they are not cited here.</p> <p>U.S. Government</p>
06	21	20		51	<p>Avoid use of specialized acronyms such as LIG and GIS. In particular, GIS has another very widely used meaning.</p> <p>U.S. Government</p>
06	21	40			<p>The 2-4°C warming in Greenland appears inconsistent with the 4-5°C warming discussed in 6.4.1 (page 17, line 20). Authors need to make sure that these inconsistencies are</p>

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					resolved and clarified. Also note that both ranges of values are inconsistent with those in the third bullet under Robust Findings on page 41. Authors should do a global search on temperatures (°C) throughout the chapter to ensure consistency among numbers and whether these are based on models or data. U.S. Government
6	021	46		51	This section is poorly written and its point is unclear. The first sentence uses past data to infer future climate and should be deleted. The entire section needs to be rewritten to specifically and clearly define the importance of last interglacial sea level. U.S. Government
06	022	43			Section 6.5.1.2: Northern peatlands have accumulated up to 450 GtC during the Holocene, which is a large portion of the (variable) terrestrial carbon inventory during that time period. This C store is large enough to have significant impact on Holocene GHG concentration variations. It is at least as important as forest regrowth and coral reef build-up. Authors should assess the role of northern peatlands (Smith et al. 2004) to determine if it is appropriate to state that terrestrial carbon has remained stable over the past 7000 years. Smith et al. 2004. Science 303: 353-356; Yu et al. 2003; Vitt et al. 2000. Can. J. Earth Sci. 37: 683-693. The Holocene 13: 801-803. U.S. Government
06	023	1			"Human activities" should be replaced by “prehistoric agriculture.” U.S. Government
06	023	4			Do not capitalize Industrial. U.S. Government
06	023	6			Insert sentence before “This hypothesis requires much larger changes....” as follows: “In a counterpoint to Ruddiman (2003), Broecker (2005) argues that during Marine Isotope Stage 11, which like the Holocene was a time of small orbital eccentricity, atmospheric CO ₂ stayed above 270 ppm for about 28,000 years (from 420 to 292 kyr B.P.). The Ruddiman hypothesis requires much larger changes....”

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					Broecker, W. S. 2005. The Holocene CO ₂ rise: Anthropogenic or Natural? <i>EOS, Transactions of the American Geophysical Union</i> 87(3), 27. U.S. Government
06	023	11		48	This finding is a bit overstated given the data limitations. The time scales of all of the Holocene warming events cited here are different than the time scale of the late 20th century warming. The last interglacial reconstructions discussed in this section are based on regional summaries that include no discussion of relative timing or justification of the underlying assumption of synchronicity. The density of the data and the relative age control among the terrestrial records during the last interglacial is not good enough to treat this period as a single response to a consistent change in climate forcing. The severe limits on dating need to be acknowledged and considered in discussions of climate responses during this period as well as all earlier periods. For the sentence starting on page 23, line 43, replace start of sentence with “Paleoclimatic data reveal that there were places, ...”. For the sentence beginning page 23, line 44, replace sentence from lines 44 to 45 with “However, current spatial coverage, temporal resolution, and age control of available Holocene proxy data limit our ability to determine if there were 50 year periods of global warmth comparable to the late 20th century.” U.S. Government
06	026	31			Intertropical convergence zone here, but referred to as ITCZ earlier in chapter. U.S. Government
06	027		33		Section 6.6.1.1 (on 2000-yr proxy reconstructions) is a little too long. It can be either shortened or reorganized into 2 or more shorter sections, say on reconstruction history, debate, and new development. U.S. Government
06	029	40		51	Two examples of mischaracterization from this paragraph follow: <i>McIntyre and McKittrick (2003) reported that they were unable to replicate the results of Mann et al. (1998).</i>

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>In fact, MM03 stated that there was “substantial success in replicating the MBH98 methodology, but some differences remain, possibly due to undisclosed variations in their procedures and assumptions.” Their specific claims were that the calculations of proxy principal components in Mann et al [1998] were “erroneous”. They concluded that the temperature indexes computed using Mann et al [1998] data and methodology were unreliable and could not be used for comparisons between the current climate and that of past centuries.</p> <p><i>Wahl and Ammann (accepted) demonstrated that this was due to the omission by McIntyre and McKittrick of several proxy series used by Mann et al. (1998).</i></p> <p>MM03 did not “omit” any series and Wahl and Ammann (accepted) does not “demonstrate” anything of this nature. On this particular topic, Wahl and Ammann only state the following: “In MM03, the authors describe this result as being developed using the MBH reconstruction methodology, albeit with elimination of a large number of the proxy data series used by MBH, especially during the 15th century.”</p> <p>There is no “description” in MM03 saying that they “omitted” several proxy series used in MBH. Quite the opposite. MM03 reported that some proxy data series said to have been used in MBH were not actually used. Subsequently, they filed a Materials Complaint with Nature, in which Mann et al. admitted that 35 series said to have been used in MBH98 were not actually used.</p> <p>Consider the following replacement paragraph: “McIntyre and McKittrick [2003, 2005a, 2005b] attempted to replicate exactly the reconstruction of Mann et al [1998] featured in the SPM of the TAR. While they claimed success in replication of the method except for some details, they also raised statistical questions potentially relevant to determination of the reliability of all reconstruction methods, highlighting the need for greater involvement of statisticians specialized in time series analysis in paleoclimate reconstructions. Firstly, McIntyre and McKittrick [2003] identified sensitivity of the 20th century warming to the presence or absence of specific series bristlecone pines, also considered by Graybill and Idso [1993] as problematic. Secondly, McIntyre and McKittrick [2005a] challenged the</p>

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					reliability of Mann et al [1998], reporting that the earliest portion of the Mann et al reconstruction did not have significant skill under reasonable ‘red’ noise assumptions. Replies by both Wahl and Ammann [2006], and Huybers [2005], agreed that the RE benchmark statistic is dependent on model assumptions, and can indicate model skill while simultaneously contradicted by r2 and CE statistics, though differ on the appropriate benchmark value for the RE statistic. These and other contingencies in the methodology were elaborated by Bürger and Cubasch [2005] who showed that plausible variations of Mann et al [1998] methodology can lead to a wide variety of results, and argued that verification statistics cannot be used to decide between models. Von Storch and Zorita [2005] also confirmed the bias towards reduced long time scale variability in the Mann et al principal components methodology. These efforts attribute the ‘hockey stick’ shape of the reconstruction in Mann et al [1998] largely to contestable statistical artifacts in the methodology, but at this point it is unclear to what extent these findings apply to other reconstructions using tree-ring proxies and principal components methodologies.” U.S. Government
06	029	41		42	The use of Wahl and Ammann (accepted) does not comply with WG1’s deadlines and all text based on this reference should be deleted. WG1’s rules require that all references be “published or in print” by December 16, 2005. Wahl and Ammann was “provisionally accepted” on that date, and not fully accepted until February 28, 2006, at which time no final preprint was available. Substantial changes were made in the paper between December 16, 2005 and February 28, 2006, including insertion of tables showing that the MBH98 reconstruction failed verification with r-squared statistics, as had been reported by McIntyre and McKittrick in 2003. These tables were not available in the draft considered by WG1 when developing the second-order draft. U.S. Government
06	029	41		42	(accepted) should read (in press), pending determination of whether or not WG1 rules regarding inclusion of peer-reviewed articles was violated or not. U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
06	030	01	30	3	<p>The “hockey stick” was one of three main lines of evidence used in the TAR to justify the conclusion that human activities were the cause of most of the warming observed during the last half of the 20th century. As noted in this chapter, this led to critical analyses of the Mann et al study. One of the criticisms of their work was the limited amount of data on which its conclusions were based. The text in this draft indicates that new studies since the TAR “... represent some expansion of the length and geographic coverage of the previously available data.” This is a weak statement, suggesting that the expansion of data has not been very great. The reader should be given more information about how much new data has been added to the analysis since the TAR, and why it justifies the strong statement that it is “...very likely that average NH temperatures were warmer than any other 50 year period in the last 500 years.” The TAR conclusion was different, assigning only a likely probability, albeit to the last 1000 years. This text indicates that there has been “some” expansion of the length and geographic coverage of proxy studies, but does not indicate how much of an expansion has occurred. This is critical information that needs to be included in the chapter. Figure 6-11 shows only scattered data for 1000 A.D, yet the Executive Summary of this chapter, the Technical Summary and the SPM all contain the conclusion that the second half of the 20th century was likely to have been the warmest 50-yr period in the last 1000 years. This will be among the most important findings in WG1’s report and the reader needs to know how much data support the finding, and whether there has been a significant increase in the amount of data available since the TAR.</p> <p>U.S. Government</p>
06	031	6		13	<p>This sentence is way too long and should be broken up into 2 to 3 sentences.</p> <p>U.S. Government</p>
06	033	18		19	<p>Instead of saying “the few new reconstructions” please give the actual number of new reconstructions.</p> <p>U.S. Government</p>
06	33	22			<p>The title of Section 6.6.1.2 (in italics) should be changed to “<i>What do ground surface</i></p>

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<i>temperature reconstructions derived from subsurface temperature measurements tell us?"</i> U.S. Government
06	33	49		52	On lines 49 and 52 there is a reference to "Smerdon et al., in press". This paper has now been published, so substitute "2006" for "in press". U.S. Government
06	34	43		44	This section is dealing with the southern hemisphere. The sentence "...these both indicate unusually warm conditions prevailing in the 20th century (Pollack and Smerdon, 2004)", and the reference therein are both incorrect. The ground surface temperature changes over the last 500 years do not indicate unusually warm conditions prevailing in the 20th century in Australia and southern Africa. This is because the unusually warm conditions developed late in the century, after most of the boreholes had already been logged. What the borehole reconstruction for Australia does show is very good correspondence with the Cook et al. (2000) reconstruction for Tasmania and the Cook et al. (2002) reconstruction for New Zealand. The Australia work is described in a manuscript "Five centuries of Climate Change in Australia: The View from Underground", by Pollack, Huang and Smerdon, accepted for publication in the Journal of Quaternary Science. The Africa work by the Pollack group is unpublished. U.S. Government
06	035	51			Should read Baliunas and Jastrow (1990). U.S. Government
06	038		41		Good to see some mention of hydroclimatic variability in this chapter, but it almost seems like a footnote. The main focus is on past temperatures and the drivers of temperature variability in order to place current warming into a long-term context and to differentiate the roles of different forcing mechanisms. In parts of North America, and perhaps elsewhere, the future scenarios for changes in moisture seem to be varied, and unlike temperature, currently clear hydroclimatic responses to global warming are not as obvious as temperature responses. As is mentioned in this section, some paleo records suggest

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>radiative forcings have influenced ENSO, which has an important influence on precipitation in many regions. In other cases, the causal mechanism for drought events and precipitation regime changes are not clear. That being said, it might be good to point out that warming temperatures, by themselves, will have large impacts on hydrology and water availability, even with no changes in moisture regimes. Breshears et al. (2005) suggest that the recent drought in the western U.S. was perhaps a taste of what is to come: global-change type droughts. Increased temperatures can alter hydrographs, change the precipitation to snow ratio, increase demand, evaporation, and evapotranspiration, and lead to persistence of drought conditions (e.g., Oglesby and Erickson 1989). If 20th-21st century warming exacerbated the recent drought, what would be the impact of this amount of warming (or more, as projected) on a drought such documented in the paleoclimatic record at the end of the 16th century? If a temperature increase was superimposed over this widespread and severe drought (which did occur during a period of generally cooler conditions), the chances are it would be even more widespread and persistent. It would be interesting to re-calculate the reconstructed gridded PDSI values for western North America (Cook et al. 2004b) with increased temperatures for this period. Maybe it would not make too much difference since temperature is a not a dominant factor in PDSI, but it might be an interesting exercise, if it could be done. As is mentioned in Section 6.6.5.5., proxy records show that the range of drought characteristics in the 20th century do not contain the full range of variability in the past 150-2000 years. These records contain evidence for relatively short droughts (4-6 years) that exceed the severity of droughts of similar length in the 20th century (e.g., the 1950s drought) as well as runs of years with below average conditions that persist for many more years than seen in the modern period. In both cases, these droughts under warmer conditions would likely result in more widespread, persistent, and/or severe events. The impacts of these paleo-type droughts under warmer conditions are apt to be far reaching, as has been hinted at with the recent drought. Just a very few mid-level water managers in the western United States, with the foresight and courage to start considering the implications of the paleo records in concert</p>

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>with the regional impacts on temperature from global warming, are beginning to incorporate this information into planning. It would be enormously helpful to bring up these points in the SPM in order to get this kind of thinking and planning elevated to a higher level of decisionmaking.</p> <p>Additionally, section 6.6.5.5 considers ENSO impacts in a manner that is far too deterministic. There is great variability in ENSO and its impacts. All El Niño events do not look like the canonical El Niño.</p> <p>U.S. Government</p>
06	038		41		<p>Add a short section on Central Asia and Middle East region. Page 40, line 24 would be an appropriate place for such a section. There is adequate proxy data for this.</p> <p>U.S. Government</p>
06	040	14			<p>This section starts by mentioning the NAO and AMO as the main sources of Atlantic variability, then spends the rest of the section talking about the NAO to exclusion of the AMO. This could be corrected by adding the following paragraphs:</p> <p>“The AMO is the leading mode of quasi-periodic, multidecadal North Atlantic SST variability related to oceanic thermohaline circulation (Delworth and Mann, 2000; Sutton and Hodson 2003; Knight et al. 2005). Over the instrumental period (1856-Present), the AMO exhibited a 65-80 yr cycle (0.4 ° C range), with warm phases at roughly 1860-1880 and 1930-1960 and cool phases during 1905-1925 and 1970-1990. The AMO appears to have returned to a warm phase beginning in the mid 1990s. AMO phases tend to be very persistent but the transitions from one phase to the other tend to occur quickly. The AMO has been associated with multi-year precipitation anomalies worldwide (McCabe and Palecki, 2006; Sutton and Hudson 2005). The AMO is thought to play a role in Atlantic hurricane formation (Golenberg et al. , Caribbean and NE Brazil rainfall, African and North American drought frequencies (Folland et al. 1986; McCabe et al. 2004), and temperatures in Europe (Sutton and Hodson 2003).</p> <p>Instrumental observations capture only two full cycles of the AMO, but a longer AMO reconstruction (A.D. 1567-1990) is now available from tree rings in eastern North</p>

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>America, Europe, Scandinavia and the Middle East (Gray et al., 2004). AMO phases in the reconstruction tend to average 20 years in duration (ranging from 9 to 53 years), except in the 18th century when AMO variability was noticeably dampened. Enfield and Cid-Cerrano (2006) estimated probability distribution functions from the Gray et al. (2004) reconstruction to calculate the probability of future shifts in AMO. AMO variability has been correlated to tree-ring reconstructions of precipitation, PDSI and fire occurrence in the western U.S. (Gray et al., 2003; Hidalgo 2004; Sibold and Veblen 2006). Correlations between AMO and winter climate in the western U.S. in both the instrumental and reconstructed record beg for a mechanism.”</p> <p>Delworth, T. L., and M. E. Mann (2000), Observed and simulated multidecadal variability in the Northern Hemisphere, <i>Climate Dynamics</i> 16, 661–676.</p> <p>Enfield, D. B. and Cid-Serrano, L. 2005. Projecting the risk of future climate shifts. <i>International Journal of Climatology</i> 10.1002/joc.1293.</p> <p>Enfield, D. B. and A. M. Mestas-Nuñez and P. J. Trimble. 2001. The Atlantic multidecadal oscillation and its relation to rainfall and river flows in the continental U.S. <i>Geophysical Research Letters</i> 28, 2077-2080.</p> <p>Folland, C.K., T.N. Palmer, D.E. Parker. 1986. Sahel rainfall and worldwide sea temperatures. <i>Nature</i> 320, 602-606.</p> <p>Goldenberg, S. B., C. W. Landsea, A. M. Mestas-Nuñez, and W. M. Gray(2001), The recent increase in Atlantic hurricane activity: Causes and implications. <i>Science</i> 293, 474– 479.</p> <p>Gray S.T., J.L. Betancourt, C.L. Fastie, and S.T. Jackson, 2003. Patterns and sources of multidecadal oscillations in drought-sensitive tree-ring records from the central and southern Rocky Mountains. <i>Geophysical Research Letters</i> 30, 49-1.</p> <p>Gray, S.T., Graumlich, L.J., Betancourt, J.L. and Pederson, G.T. 2004. A tree-ring based reconstruction of the Atlantic Multidecadal Oscillation since 1567 A.D. <i>Geophysical Research Letters</i> 31, L12205, doi:10.1029/2004GL019932.</p> <p>Hidalgo, H.G., 2004. Climate Precursors of Multidecadal Drought Variability in the</p>

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					<p>Western United States. <i>Water Resources Research</i> 40:W12504:10 p.</p> <p>McCabe GJ, Palecki MA (2006) Multidecadal climate variability of global lands and oceans. <i>International Journal of Climatology</i>. DOI 10.1002/joc.1289.</p> <p>McCabe, G. J., Palecki, M. A., and Betancourt, J. L. 2004. Pacific and Atlantic Ocean influences on multidecadal drought frequency in the United States. <i>Proceedings of the National Academy of Sciences</i> 101, p. 4136-4141</p> <p>Sutton, R.T. and D.L.R Hodson. 2003. Influence of the ocean on North Atlantic climate variability 1871-1999. <i>J. Climate</i> 16:3296-3313.</p> <p>Sibold, J.S. and T. T. Veblen, 2006. Relationships of subalpine forest fires in the Colorado Front Range with interannual and multidecadal-scale variation. <i>Journal of Biogeography</i> 33, 833-842.</p> <p>Sutton, R. T., Hodson, D. L. R. 2005. Atlantic Ocean Forcing of North American and European Summer Climate. <i>Science</i> 309, 115-118.</p> <p>U.S. Government</p>
06	041	7		8	<p>Decide on the wording of finding about the NH temperature during the 20th century. In the Executive Summary and in WG1’s higher level summaries, the finding is that the second half of the 20th century was the warmest 50 year period in the NH in the last 1000 years, and unusually warm compared with the last 1300 years. The Executive Summary also states that the regional extent of NH warmth was very likely greater during the 20th century than any other century in the last 1300 years. Page 6-33, lines 19-20, state “... it is likely that (in the NH) the 20th century was the warmest in at least the past 1300 years.” This table states that “it is also likely that this was the warmest 50-year period in the past 1300 years.” While these three wordings are similar they have non-trivial differences that should be resolved. A concerted effort needs to be made to ensure consistency within Chapter 6 and among IPCC chapters when making statements of this sort.</p> <p>U.S. Government</p>
06	041				<p>Third item should read “Global sea level rise due primarily to ...”. Also there is</p>

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					disagreement between this temperature range and those listed elsewhere in the text. Fourth item should read “associated with”, not “linked to”. Linking infers causation. Eighth item needs clarification because dry periods last decades to centuries and droughts do not. Last item should read “Models are capable of simulating many aspects of climate and vegetation change for past periods of different forcings.” U.S. Government
06	041				Last item under Key Uncertainties should read “The lack of extensive networks of proxy data that are resampled and updated to the present day means ...”. Proxy data do not run. U.S. Government
06	052				Should read Esper et al. (2005). U.S. Government
06	064	4			Change “in press” to “2006” U.S. Government
06	064	6			Delete “In press.” Add “ <i>Journal of Geophysical Research – Atmospheres</i> , 111(D07), art. no.-D07101.” U.S. Government
06	071	17		19	Needs to be rewritten to make it clear that (1) prior to ice cores we cannot measure rates of climatic change comparable to today’s, and (2) for that reason we have no evidence if similar rates were seen before 600,000 years ago. The current text seems to imply that past rates were not as fast as today. Additionally, most older paleoclimatic records are from single points. This makes global-scale inferences questionable. U.S. Government
06	080				For Figure 6.1, middle and lower panels, scale lines on Y axes need to either be outward (as in the top panel), or at least as a layer above the data so that they are not covered. Many of these figures might benefit by the scale lines graphed outward rather than inward. U.S. Government
06	084				In Figure 6.3, the meaning of the three stars at the top right is not explained.

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
06	097				Figure 6.13. The thick lines are not recognizably different from the thin lines. Suggest that either the thick or thin lines also be made dashed. U.S. Government
06	097	10			Figure 6.13, legend, line 10. In the parenthetical statement “(modified from Figure 6.11c)” you really mean Figure 6.10c. U.S. Government
07	000				This chapter needs serious editing and much more attention needs to be paid to the figures. Much of what is said is ambiguous and confusing and it is difficult to discern the high points in the text. Attention needs to be paid to detail, especially in Sections 7.1 and 7.2. U.S. Government
07	003	25			Add another bullet describing what happens to the other 55% of the CO2 besides the 45% described on line 21-25. Section 7.3 has an excellent discussion on how some CO2 will exchange with the deep oceans and how some of the CO2 perturbation will extend for thousands of years. U.S. Government
07	003	56			A lack of consensus does not point to a particular answer. We suggest: removing “since” and adding “however after “TAR” U.S. Government
07	004	49		52	This bullet should be deleted. The statement that “[organic] emissions are expected to increase in a warmer climate” is only partly supported by the discussion in the chapter on pages 7-53 line 33ff and 7-60 line 57ff. Those sections say that the expected increase in organic emissions with temperature may be partially negated by other factors such as ecosystem response. The statement “marine biochemistry may also be a source for organic aerosols” is true but is too vague to merit inclusion in the chapter executive summary. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
07	009	16			The point of the last sentence in the paragraph is not immediately apparent. If it is meant to be an example of observations that can help guide choices, we suggest beginning sentence with a linking word or phrase (e.g., "Examples of such guiding observations are...."). U.S. Government
07	009	39			It is unclear what is shifted. The system? U.S. Government
07	013	32			If there is a disagreement among observations this should be explicitly mentioned in the text to avoid confusion about the point and guide the reader as to any degree of consensus or controversy. It would also be helpful in that case, as may be the purpose of the last sentence, to go over possible reasons for the observed differences. U.S. Government
07	013	40			Albedo and emissivity are not contributions to radiative balance; they are mechanisms or characteristic quantities of mechanisms. Also, correct number (albedo and emissivity are plural). U.S. Government
07	028	18			Is CO correct here, or should it be CO ₂ attributable to wildfires. U.S. Government
07	037	47		48	It is unclear where these time scales come from. The only similar statement was on Pg 7-35, lines 54-55: "This slow approach to a new equilibrium takes 30,000-35,000 years." U.S. Government
07	037	47		48	Where do the 5,000 year, 10,000 year, and 40,000 year time frames come from? Pg 7-35, lines 54-55, gives a time scale of 30,000-35,000 years for an approach to a new equilibrium, which doesn't match any of the time scales in this "robust finding." U.S. Government
07	046	29		30	This statement appears to contradict that on page 7-44, Line10. U.S. Government
07	047	08			It might be good to state explicitly early on that N ₂ O is a greenhouse gas. The section

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					should also note that N ₂ O is not a reactive form of nitrogen, as are the other species here. Nitrous oxide alone is the fourth largest single, long-lived contributor to radiative forcing. Its role should not be confused with the others. U.S. Government
07	049	21		22	A short lifetime does not dictate but springs from the primary mechanism for removal. U.S. Government
07	049	40			It’s confusing to read several statements indicating that N is limiting and that N addition leads to increased plant growth, and then to read that added ¹⁵ N is not taken up after seven years. How can this be? U.S. Government
07	051	19			We suggest providing ozone lifetime in the upper troposphere relative to elsewhere in the troposphere. U.S. Government
07	055	49			Section 7.4.5.4 contains several digressive passages. Suggest revision to increase conciseness and focus on the topic at hand. U.S. Government
07	59	53	59	53	The correct reference is Prospero and Lamb (2003) and not Chiapello et al. (2005), as the latter study is based on the measurements and analysis of the first. Prospero, J. M., and P. J. Lamb, African droughts and dust transport to the Caribbean: Climate change implications, Science, 302, 1024-1027, 2003. U.S. Government
07	060	04			We suggest including a phrase describing the radiative effect of dust considered here (e.g. assume uncoated dust’s direct effects are referred to; land vs. ocean?), as well as an indication of whether the feedback in question is positive or negative. U.S. Government
07	061	20			The phrase “the organic contribution” refers to what? Possibly the biogenic contribution to organic matter?

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					U.S. Government
07	063	19			Table 7.5.1 is inconsistent with Chapter 2 in terms of what is included in radiative forcing. U.S. Government
07	065	27			Add reference to measurements of aerosol effect on changes in cloud convection (e.g., Koren et al. GRL 2005; Koren et al. Science 2004). U.S. Government
07	066	22		30	Authors should provide an estimate of black carbon particle emissions from aviation (along with uncertainties). The study cited is hypothetical and the summary included in AR4 needs to quantify. U.S. Government
07	067	3		4	The phrase, “the change in net radiation at TOA from pre-industrial times to present day” is defined as radiative forcing in this Assessment. Chapter 2 excludes the cloud lifetime effect from radiative forcings. Reconcile this difference. U.S. Government
07	067	43			Remove the phrase “solar dimming” from this sentence. U.S. Government
07	068	18		34	This is a very important paragraph that deserves more emphasis, either by setting it off in a sub-section or by mentioning it in the executive summary at the start of the chapter. Buried in this paragraph is the concept that, unless there is strong ice indirect effect, a very large aerosol indirect effect would be inconsistent with the observed increase in precipitation. By providing circumstantial evidence against a huge negative aerosol indirect effect this concept adds confidence to the statements in Chapter 2 that humans have very likely exerted a warming influence on climate. This paragraph could mention and be coordinated with section 9.5.4.2.1 on attribution of changes in precipitation. U.S. Government
07	122				In Figure 7.1, make “chemistry/aerosols” arrow double-headed, including an indication of heterogeneous chemistry (e.g., “gas/aerosol reactions,” “precursors,” “catalyzations”). This

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					link is what makes CFCs so devastating to the ozone layer. U.S. Government
07	123				Label axes in Figure 7.2.1. Expand on this plot: is the variability only in precipitation intensity? Where does the leaf water come in? At minimum state the implications of the results. Reconcile American and UK spellings. U.S. Government
07	124				Figure 7.2.2. We suggest linking this figure to the previous one. Suggest linking “realistic” and “variable” in the discussion. Suggest using the same type of plot (line or bar) for easier direct comparison of the two figures. Also, label the vertical axis. U.S. Government
07	125				Figure 7.2.3. Expand on this figure. What are the three sets of observational estimates? There are nine plots with different labels; what do these mean? What do the arrows mean? Suggest including a sentence explaining the significance of the figure. In particular, it appears that the order of magnitude difference between models appears in only one or two instances; these might be highlighted. U.S. Government
07	126				Figure 7.2.4. We suggest pointing out features of particular interest in the plots. Suggest pointing out that the top two rows show some additional information (possibly an average of each column and some other value called “rep”) and what these are. Finally, suggest text to explicitly compare this figure with the previous one to aid the reader in understanding the significance. U.S. Government
07	127				Figure 7.2.5 This figure and caption could use some work to increase the impact and ease of interpretation. Is the word “causes” appropriate here? In the text Hadley center model is described as having weak coupling. Which one? What is the vertical axis? What is an averaged coupling? For that matter, what is coupling itself – units? U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
07	130				Figure 7.3.3 We suggest not including the information about how the data has been processed in the vertical axis (“(SPO+MLO)/2”) but placing this information in the caption. Not clear what the information in parentheses is (“{1, –“, etc.). In-caption key? Suggest including a key or annotating the figure. U.S. Government
07	131				Some of what needs to be in the caption of Figure 7.3.4 appears in the text. We suggest moving some of it here and repeating some of it here. State what the line in the plot represents (best-fit,?). Give the vertical axis label in words or use the symbol from the main text ($\Delta\text{CO}_2^{\text{N-S}}$). We further suggest giving a sense of the relation of this correlation to time (second horizontal axis?). Suggest stating the significance of this plot in the caption (i.e. the hemispheric distribution of emissions and its implication for sources of carbon to the atmosphere). U.S. Government
07	132				Figure 7.3.5 Are thick black lines described as grey in the caption? Suggest treating the “method 1/method 2” issue in the caption. U.S. Government
07	132				For Table 7.3.5, we suggest naming the models as is done in the previous table. U.S. Government
07	133				Figure 7.3.6 This figure and caption need work to improve clarity and visual impact. Some specific suggestions: (1) make consistent use of top-down and bottom-up terms (consistent with the text); (2) improve contrast between different colors in the plots (e.g. red and orange are too similar); (3) make the quantities plotted consistent throughout the figure (e.g. land plus ocean inversion fluxes), or explain why not in the caption; (4) use a color key rather than use the names of the colors in the caption (it is more difficult for the reader to visualize “cyan” and then look up to find it in the plot). U.S. Government
07	138				Figure 7.3.11 Hyphenate “ocean-only runs” (Line 6). Good figure and caption. Use a single

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					scale for the vertical axes to show that the amounts of CO2 input into the model are varied; otherwise, to the reader it looks pretty much like the same plot eight times. U.S. Government
07	138	2			Figure 7.3.11: This is a good figure, but there is no corresponding figure in the chapter that shows the response of the earth system to carbon dioxide (loosely, its lifetime) on a more human time scale. We recommend shortening this figure from 8 panels down to one or two and adding a panel, from this or another peer-reviewed paper, showing, over a time scale of hundreds of years, the effect of a CO2 injection on the atmospheric concentration of CO2. U.S. Government
07	145				Table 7.4.4 Label the terms as either sources or sinks of tropospheric ozone. Are there any indications of uncertainty or error in these figures? Does “Burden” indicate the balance in the troposphere? How is this derived? U.S. Government
07	149				Figure 7.5.1 This is a good figure. Suggest adding a noun after “schematic” (e.g. drawing). U.S. Government
07	150				Figure 7.5.2 What are the grey lines and boxes? Do the percentages add up to 100% of dust generated in this region? Give complete flux units in the legend; I had to scrutinize the caption to find out whether they were cumulative or annual or seasonal averages. Since deposition is mentioned, it might be best to mention that much Asian dust is not deposited back into Asia, but is transported on continental scales (important, since it appears to be a continuous source and dominate background dust aerosol on the west coast of the US, see Cliff et al recent work). U.S. Government
07	151				Figure 7.5.3 It seems confusing to make the higher-albedo cloud a darker color. Suggest placing a noun after “schematic”. Avoid (here and throughout the document) use of the

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
					term "solar dimming". U.S. Government
07	155				The caption needs quite a bit of unpacking; we assume that is planned U.S. Government
07	156	2	157	2	Figures for Box 7.4. Both Figure 1 and 2 of Box 7.4 convey the same message. Choose one or the other. U.S. Government
08	00				General comment on section 8:2: since this is really a discussion for IPCC, what is the rationale for spending time discussing model improvements that are not used by the models in their IPCC formulations? It is all well and good to look to the future of modeling, but that's not really the role of this chapter, and in some sense it is misleading - it seems to imply model capabilities that are not actually being utilized. This comment does not apply to modeling studies that point out the value or deficiency of some modeling component that is not being used in the IPCC simulations. U.S. Government
08	00				Should include a table that shows what changes are in AR4 models compared to those in the TAR - and perhaps a separate column that indicates what advances are occurring in models not used for IPCC assessments. U.S. Government
08	00				This chapter would benefit greatly from a table of robust findings and key uncertainties, similar to the ones that appear in some earlier chapters. U.S. Government
08	00				This chapter should include a table of robust findings and key uncertainties, rather than expecting the reader to extract them from the text. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
08	004	42			Which formulation? U.S. Government
08	005	4			Is this also to imply duration of extreme events, and in that case what about blocking, which is indicated later to be underestimated in duration? U.S. Government
08	005	14			qualitative inferences about... U.S. Government
08	005	31	14	34	Should be deleted. Refers to model capabilities outside the IPCC framework. Could be reformulated to say that IPCC models in general have not been assessed for these purposes (and assessment itself might be difficult - perfect models do not imply perfect skill). U.S. Government
08	007	46			within the envelope of internal variability U.S. Government
08	008	52	008	53	but there are only a few preliminary studies and the inferences one can draw from the whole approach are not clear. U.S. Government
08	010	48			In the whole paragraph, numeric changes were characterized in previous paragraphs (line 30) as improvements; this paragraph seems somewhat equivocal as to whether they really are improvements. U.S. Government
08	014		15		Should be deleted. Refers to model capabilities outside the IPCC framework and is misleading by implying that these aspects feed into IPCC results. U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
08	015	5			While bucket models may be worse, that doesn't indicate current land surface models are adequate. The response of vegetation and soil moisture to increasing temperature - the sensitivity of ET to warming - is still quite uncertain in models, and differs greatly between GCM land surface schemes and those used in Impact Models (which has in the past contributed to big differences in projections of future water availability changes between IPCC WGI and WGII) [and note the discussion starting on line 19 which points out that problems remain]. U.S. Government
08	017	20			Another example of a discussion of models that are not used for AR4. This refers to the whole section 8.2.5. U.S. Government
08	018	22	18	22	"... may in fact be linear to first order,..." Is not everything linear to first order? Perhaps use "approximately linear in response to modest forcing..." U.S. Government
08	018	29			Nevertheless deficiencies in simulating the current climate could indicate U.S. Government
08	018	42			Comment about most of the following discussion being focused on CMIP models is not followed in the subsequent discussion (e.g., 8.3.1.3, the land surface discussion, etc.) U.S. Government
08	021	50			The Pacific ITCZ in general does cross into the S.H., so the 'explanation' should be modified. U.S. Government
08	022	41			Provides one piece of information on extratropical cyclones U.S. Government
08	027	1			Probably should include vertical mixing in the ocean in this paragraph, which is a dominant influence in the S.H. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
08	027	15		37	Some models get too much snow in spring, some get too much ablation in spring, some get good seasonal variation, some don't - it's quite confusing. Again the focus should be on the models used for AR4. U.S. Government
08	027	49	28	2	Another example of a discussion of model results not relevant to AR4. Was Stitch et al. (2003) used for AR4, and if so, how badly did it do? U.S. Government
08	028	10			Since both solar radiation and atmospheric composition (including aerosols) are somewhat uncertain, perhaps the better comparison is with climate changes over the 20th century. U.S. Government
08	029	39			how model climatology has evolved U.S. Government
08	029	45			This should really be AR4 models, but even more, these are not all coupled models (or even all models) being discussed, despite the subchapter title. U.S. Government
08	030	55			NCEP reanalysis used in this comparison [could have used ERA40] U.S. Government
08	031	51			Does the 'poor resolution of the coastal wave-guide' refer to the models, or the observations? This should be specified. Use "...coastal wave guide in models." U.S. Government
08	035	19		35	This is not at all relevant to AR4 models or the rest of this section, and should be deleted. U.S. Government
08	037	31		44	Should be noted that these comparisons are being made with observations of precipitation, which have large differences between different observational data sets. U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
08	037	46	38	34	This section should be removed. It is not relevant to AR4 models (perhaps by the time of AR5 it will be relevant). U.S. Government
08	038	19			Which 6 months? It is well known that given the initial conditions for April, ENSO state forecasts for December can be well done; but given the initial conditions for December, April conditions are very difficult to forecast. Is that true in these studies as well? If so, then this aspect is misleading. If the 'six month forecast' statement is meant in general, then these models are doing better than models specifically designed to forecast ENSO conditions, often with much finer resolution. This also raises the more general question: how far removed is GloSea from the models used for the IPCC assessment? If it is much different, e.g., much finer resolution, than this chapter has to be careful not to mislead readers into thinking that the results are relevant for this IPCC report. U.S. Government
08	039	28			But doesn't the run use 'ocean forcing', aka prescribed SSTs? Then whether the effect is caused by ENSO or not, the model is not producing the proper extreme response. U.S. Government
08	040	55		57	Should remove this sentence - it's a policy-related (or at least WGII related) concept. U.S. Government
08	042	32			This statement should be removed - model sensitivity is an issue in model development, perhaps not in the initial implementation of subroutines but certainly in the assessment of the model before it is released. U.S. Government
08	045	16			Show a range of results U.S. Government
08	049	9			Statement is inaccurate; low level clouds exist immediately before a warm front, not only in regions of descent. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
08	049	29		49	Paragraph is confusing. If CRF approach shows half of the models having a positive and half a negative feedback, and PRP shows them all positive, is what is meant by the two approaches are 'well-correlated' is that their relative ranking (which is most positive, which is less positive, etc.) remained the same? Perhaps this could be said more clearly. The last phrase 'similar range of magnitude' is also quite confusing, given the positive versus negative differences in the two approaches. U.S. Government
08	052	29		46	Climate metrics should also include the simulation skill of the AR4 models for the 20th century (it could be right for the wrong reasons, but nevertheless, it is a test). U.S. Government
08	054	57			Note that there is also the potential for NADW changes to instigate changes in the deep water formation around Antarctica, with potential impacts on Antarctica - see Rind et al., 2001 (reference already listed). U.S. Government
08	056	4		7	The paleo-perspective is useful here - abrupt climate changes in the paleorecord are in general associated with only small changes in atmospheric CO ₂ . U.S. Government
08	057	16			General comment on EMICs: in response to question 8.1 (p.91), the first source of confidence listed for climate models is that they solve the fundamental equations for conservation of mass, momentum, and energy (as well as moisture). To the extent that EMICs violate this condition, they cannot be thought of as supplying a confident numerical conclusion regardless of how well they can reproduce results from GCMs - simulating the right result for the wrong reason does not improve a model's reliability. U.S. Government
08	091	3		5	Plausible quantitative estimates within a range (still a factor of 2 to 3 in climate sensitivity even on the global scale). U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
08	091	30		31	Unfortunately, models cannot simulate the proper amount of ice age cooling because we really don't know what that is (we don't know how cool the tropics, or half the globe, really were).. In fact, no climate model has produced, on its own, an ice age climate, without specification of boundary conditions a priori. And the mid-Holocene warmth is due entirely to enhanced solar insolation over northern latitudes during summer - that is not really a test of models. U.S. Government
09	00				This chapter has a tendency to list various studies without providing a clear synthesis or overall assessment of the issues. The approach appears in many of the paragraphs. It is very important that this attribution chapter provide this expert analysis, using the IPCC statistical meanings of likely, very likely, etc. If no such judgment is possible, then that should clearly be stated. Examples are given below, but an overall review of the chapter should be conducted with this in mind. U.S. Government
09	00				In the figures, there is an uneven distribution of how many models are used for each point. It is awkward, and raises questions of why certain models were picked to illustrate which point. Furthermore, comparisons of the results between the different figures is impossible under these circumstances. U.S. Government
09	00				Here, and through the IPCC report in general, the \pm values should be noted as implying one standard deviation, or 'likely'. U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09					External, internal, natural and anthropogenic are used in confusing manners. Simplification would be better. The primary distinction should be between anthropogenic and non-anthropogenic. To say 'external anthropogenic' forcing is confusing, since the anthropogenic forcing is internal to the troposphere. U.S. Government
09	00				Consider whether the summary for each subsection should be in the front of each subsection (like the Executive Summary is in the front of the chapter). U.S. Government
09	003	8			Some of the greenhouse gas warming was likely offset by other factors (e.g., aerosols). U.S. Government
09	003	10		11	The basis for saying that the temperature changes of the past half century took place at a time when natural forcing would be expected to have produced cooling is far from clear. Section 2.7 (Pg 2-53, lines 39-53) indicates no trend in the past 25 years in solar irradiance. The section also discusses two major volcanic eruptions (El Chichon in 1982 and Pinatubo in 1991), but then concludes that stratospheric aerosol concentrations are at their lowest concentrations since the satellite era and global coverage began in the late 1970s (Pg 2-58, lines 55-56). These facts would seem to indicate no effect of solar variability and a declining effect of volcanic activity for that period. A greater exposition should be given about what 'likely' truly means in this context, and whether these numbers can be truly quantified over the last 50 years given the uncertainties in both solar and volcanic aerosol reconstructions. U.S. Government
09	003	10		11	Are natural external causes and non-anthropogenic external forces the same thing? Why use different terminology? U.S. Government
09	003	55		56	“likely related in part” is less than transparent language. Reword. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	004	09		12	Sentences beginning “The observed....”, and ending “...factors”, are vague. Rewrite and remove last sentence. U.S. Government
09	004	18		19	“A large fraction....” Be more specific. Don't know natural variability or solar variability, so 'a large fraction' and 'very likely' seem too strong. Better to say it was very likely not the result of anthropogenic forcing U.S. Government
09	004	27		28	This is a strong statement...this is not really constrained by observations. It is simply an extrapolation and a model result. U.S. Government
09	004	30		32	“...many observed changes....are distinct from internal variability”. This sentence lacks specificity, and is too strong in tone. This is too strong a statement to be left this ambiguous - be specific, which changes in particular? And what uncertainties afflict them? Does internal variability include, in this context, natural forcing as well? U.S. Government
09	005	10			“...attempt to place that work...” It is not clear what “that” refers to? What previous assessment precisely is being referred to here? U.S. Government
09	005	12			Should it say “quality of available climate models” instead of “available climate models”. Also eliminate 'sometimes'. U.S. Government
09	005	17		19	Definition of climate change needs improvement. For example, the word “change” is utilized twice in the definition of climate change. Needs improvement in the glossary as well. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	006	16		18	This relationship is very regionally dependent - e.g., warming and wetter conditions occur at high latitudes due to advection from lower latitudes. Probably should be removed, or at least have clear caveats. U.S. Government
09	006	8		18	This whole paragraph does not really add anything of substance. U.S. Government
09	006	28		30	The example doesn't really help. Perhaps say, "Extreme events can occur in an unchanging climate." U.S. Government
09	006	50		51	Drop (i) and move the other two components up to (i) and (ii), and in (i) change to 'demonstrate that a detected change is...' U.S. Government
09	007	05			Not sure if it should say "consistent" or "inconsistent". This sentence should be greatly simplified, along the lines of "the observed change is inconsistent with alternate hypotheses." U.S. Government
09	009	42		45	Is all model analysis related to "radiative forcing"? Are there no model simulations that attempt to attribute changes to non-radiative anthropogenic influences, for example, anthropogenic changes in land cover. Line 40 should indicate, as the chapter title does, of climate response to radiative forcings. U.S. Government
09	010	03		4	Is it implicitly saying that the responses are linear and can be scaled? Could any references be provided. Are there model simulations and analysis that substantiates this statement? Could give the (Meehl et al 2004) reference here, but an overall consideration of how general this comment is should be considered. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	010	33		34	In its FOD, Chapter 2 did estimate total net forcing. That estimate has been deleted in this draft. If it is necessary for Chapter 9 to use a net value, it should be indicated as Chapter 9's interpretation of Chapter 2's information, not attributed to Chapter 2. U.S. Government
09	010	33		34	How was this estimate of total net forcing derived? It does not appear in Chapter 2's discussion of radiative forcing. U.S. Government
09	011	01		8	Clarify and simplify the paragraph. U.S. Government
09	011	13	11	15	Awkward sentence. Also, what is special about ice phase? Is this the only problem climate models have? An alternate version could be "...forward calculation has not evaluated all forcings and feedbacks." U.S. Government
09	011	25			Also assumes ocean heat uptake is correct (for transient response) U.S. Government
09	011	35		45	Very un-illuminating paragraph....makes for needlessly difficult reading. U.S. Government
09	014	37			...and primarily due to the strength of the feedbacks relative to the initial forcing. U.S. Government
09	016	14		15	Should the sentence say "... It has been shown that for a range of forcings, the global ..." The way it is written sounds like difference forcing agents have same per unit response. Clarify the sentence. U.S. Government
09	017	2		4	"..., this does not strongly affect estimates ..." In line of comments made prior to this, i.e., spatial pattern of greenhouse warming and aerosol cooling cannot be distinguished, how is this statement justified? Issue and sentence should be clarified. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	018		20		<p>Section 9, pages 18-20 discuss climate change trends over the past 1000 years. It notes that 1675-1850 was a period of very cold temperatures, well below the average for the millenium. It further acknowledges that "it is not possible to simulate the large warming during the 20th century without anthropogenic forcing, stressing the importance of human activity to global warming." The second clause in that sentence (after the comma) reminds me Type I statistical errors. If the null hypothesis is "climate change is substantially caused by natural forces," then the report is rejecting that null. But what if that null is true? The report does not appear to look for the causes of the cooling during the 1675-1850 period. If there was not an anthropogenic cause for the historical cooling, what was the cause? Could it be that there were non-anthropogenic forces at work prior to the Industrial Revolution and the instrumental age of temperature measurement? Further, if human-activity-focused climate change models cannot retrocast historical climate changes, then why are we so confident in them? If we cannot answer these hypotheses, then our Type I error of "a jury convicting an innocent defendant" has extremely costly and disastrous consequences. The basic contention is that natural variability has an unknown magnitude that could have produced the colder conditions in the past and have some influence in the present.</p> <p>U.S. Government</p>
09	018	37		47	<p>There is still little understanding of the tropical response during the LGM. Cooling of 2°C of the global oceans is not sufficient to reproduce the land evidence, which implies cooling on the order of 5°C. This lack of consensus on tropical understanding is a very important feature - failure to mention it in these paragraphs makes them highly misleading.</p> <p>U.S. Government</p>
09	019	1			<p>Change "most notable changes in climate" to "most notable indication for changes in climate"</p> <p>U.S. Government</p>
09	019	29		30	<p>Should say "...less frequent and less intense..." if that is what is meant.</p> <p>U.S. Government</p>

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	019	33			Should it say “A key element for less frequent and less intense ENSO is the Bjerknes mechanism.” Clarify which ENSO response is being referred to. U.S. Government
09	019	34			This paragraph buys into the idea that warming will be greater in the western than eastern Pacific due to the importance of dynamics in the eastern Pacific. However, in Chapter 10 and elsewhere, results are shown indicating the majority of coupled models respond to warming (from GHG) by warming the eastern Pacific more than the western. This paragraph thus inadvertently contradicts the future model projections. This topic remains controversial, and by wholeheartedly embracing one particular conclusion here, it puts this section at odds with other parts of the report. U.S. Government
09	021	24		25	Should include some references from literature on decadal variability of ENSO. Generalize the community opinion. U.S. Government
09	024	12		14	Most of these studies use a relatively large solar forcing component; that should be mentioned. U.S. Government
09	024	32		41	The authors rightly note that the ability of climate models to reproduce observed temperature trends in the 20th century may be a function of improved models, or a fortuitous occurrence caused by compounding errors. This important point is now much better referenced. However, the implications to policy makers should be discussed in the summary. Should be noted in summary on p. 38, but this chapter is not discussing policy implications. U.S. Government
09	026	16		57	Too wordy and involved. Be more succinct and pointed. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	026	38			The previous paragraphs give a range of greenhouse warming for the 20th century of 0.6 to 1.5°C - how has this constrained their warming? U.S. Government
09	027	25			It was noted earlier that the SH has warmed less than the NH; hence the use of the hemispheric temperature contrast by itself would imply aerosols were a positive forcing. Heat uptake by the ocean has to be taken into account here. U.S. Government
09	027	31	28	41	Too detailed. Is all this discussion, too often very technical and specialized, needed? U.S. Government
09	027	43		46	Doesn't this para and that of page 28, lines 9-16, basically make the same point? U.S. Government
09	028	1		57	Could this be made into a separate box, and just the summary statements provided here? Or perhaps use here more of what is on p. 96 and move the complexity to the appendix. U.S. Government
09	028	48			Unlike weather forecasting, the model errors are likely also the result of uncertainties in the forcing, hence predictions assuming they are only model errors are unlikely to be realized. This affects the 2.8°C max value assumed for the transient response at the time of 2CO ₂ doubling. In addition, there are many different data opportunities for weather forecasting; using too limited a data set can produce worse (MOS) results. These caveats might lessen the strength of certain sentences in this paragraph. U.S. Government
09	029	11		20	Of course this assumes model accuracy in the latitudinal structure, etc of the warming (and models have quite different latitudinal structures of warming for the next century). Also, maybe natural and anthropogenic forcings could have similar regional/latitudinal structures. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	029	32			Chapter 3 also notes that the 'observed' increase in cloud cover from surface observers contradicts the conclusion from satellites. U.S. Government
09	029	36		56	Too many “could haves....” and far too convoluted of a discussion. Get to the point.....succinctly U.S. Government
09	030	1		42	The focus on response to individual forcings is given far too much emphasis. One loses a sense of an order of importance here. The lengthy discussion of isolated effects of individual forcings leads to a “losing the of rest among the trees” syndrome. The main emphasis should be upon giving a clear message of net anthropogenic effect. Perhaps one should add, at the beginning of this page, a comment concerning the estimated net anthropogenic forcing before going into the details, so as to provide perspective. U.S. Government
09	031	30		34	Too much detail. Simplify to make the main point. Also the phrase 'natural internal variability' appears. See SPM comment regarding this usage. U.S. Government
09	032	28		34	Where a key point is being made, start the paragraph with it. So, in this para, begin with “Evidence now exists for a likely human influence on regional climates”, rather than having that key statement be a trailer. There are other examples, in this Ch 9, of such poor paragraph structure. U.S. Government
09	032	38		40	Be more concise. This lengthy sentence better broken into two. U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	032	41		42	Here, and elsewhere in Ch 9, it becomes distracting when particular models (HadCM3, GFDL CM2, etc) are singled out. Is this always, or at all, necessary? No reason for these particular models to be discussed, positively or negatively - better to discuss the general model results. U.S. Government
09	032	52	33	13	Too much detail. It would suffice to state that optimal detection methods have been applied to subcontinental scales, without detailing the results of each one. De-emphasize the detail on the techniques. U.S. Government
09	033	15		38	Again, too much review detail. There is ample room for shortening. U.S. Government
09	034	25		35	This paragraph is anything but clear. A re-write is needed. A figure exists in the technical summary (TS 3.4, Fig. 1) showing an idealized pdf of changes in temperature extremes that could be referred to (or included) here. Also, instead of saying 'underlying deterministic changes, one could just say 'mean' changes. U.S. Government
09	035	15		18	Similar evidence exists from the analysis of variability on seasonal time scales. References include: Zwiers et al. 2000, JGR, 105, 7295-7315; Kumar et al. 2000, Journal of Climate, 13, 3139-3151. Also note that previous work, quoted in previous IPCC reports, have discussed reduced temperature variability as the latitudinal gradient decreases. The lack of change in variability is not a general result for all time--scales and all regions. U.S. Government
09	35	47		50	If the data isn't good enough to conclude anything from 1979 to the present, how can we really conclude anything from 1958 to the present? U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	036	31		40	While the info in this paragraph is appreciated, this section should be initiated with a statement of the main point regarding the “robust detection of anthrop. Influences on the free atmosphere” U.S. Government
09	037	17		26	The role of observed SST changes in global terrestrial warming has not been adequately represented here. A recent EOS study, that appeared 9 May 2006, indicates the vast majority of terrestrial warmth observed in 2004, one of the warmest yrs in the instrumental record, could be explained as a response to the observed SST forcing. Result is overstated (due to the dominant influence of SSTs on a good portion of the tropospheric profile) and the lack of specifics about how the temperature profile really has been improved by addition of anthropogenic forcings. How much did ozone do, how much did CO ₂ do - and how well do we really know the vertical temperature profile change anyway, especially in the tropics. U.S. Government
09	037	21		22	Does the study cited based on a single model (from the reference it sounds like that)? There is other SST forced AGCM analysis where observed 200-mb heights are well reproduced (see Kumar et al. 2004, Journal of Climate, 17, 653-. Does the statement also imply that the direct influence of radiative forcing large? U.S. Government
09	037	31		36	There appears to be a discrepancy here - in the tropics, is there too much tropospheric warming compared to the surface or not? U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	038	05			The summary is excellent. Suggest that it lead the section, rather than trail the section. This is true for all the major summaries in the Chapter 9. The detailed literature reviews and evidences can follow the summaries, for those interested (which, after all, will mostly be the technical experts, and not decisionmakers). Is this structure an IPCC requirement or are the chapters free to place their summary statements where they want? Giving the highlights ahead of time often makes the following reading more comprehensible. U.S. Government
09	38	29			Is the word 'likely' here used in the IPCC sense (hence, not 'very likely')? U.S. Government
09	38	40		42	This sentence is obscure; better to add at the end of the previous sentence, 'partially offsetting greenhouse gas warming'. U.S. Government
09	39	23			This sentence appears meaningless. U.S. Government
09	039	39		41	No reason to begin this section of Ocean State Changes with “prognostic” language such as “warming of the surface should lead.....”, “melting glaciers will....”. Just state what the changes HAVE been, and then give the understanding of attributable causes. I suggest beginning the para with the sentence “The overall heat content....” U.S. Government
09	040	29			The phrases natural internal and natural external variability are used; in addition, the words 'observed signal' are used - the observations contain more than just 'signal'. U.S. Government
09	040	4		25	This paragraph could be greatly reduced by eliminating details of each individual paper, and focus on the main message common across them. Provide main message, and then provide an occasional reference to back it up. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	040	57			Strong statement about the increased in the global hydrologic cycle based on one reference. U.S. Government
09	041	17		38	Recommend beginning the paragraph with the essential attribution statement, namely as line 37 reads “It is not yet possible to attribute changes in the MOC properties to natural and anthropogenic causes” U.S. Government
09	041	20			The caveat from the paragraph above is applicable here as well. U.S. Government
09	041	42		57	Give the relevant attribution knowledge in the lead sentence, rather than the procedural statement of what models must do, etc. U.S. Government
09	042	1			Really only one large volcano in the 1960s, but several large volcanoes over the last 50 years. U.S. Government
09	042	31			Is the model's contribution here just to give the temperature change globally? Could say temperature results from the AOGCMs imply... U.S. Government
09	042	32			+ between 1993 and 2003 is a mistake. U.S. Government
09	042	49			Who is the “we” in “we obtain...”? Remove the personal pronouns. U.S. Government
09	043	18		20	“the extent to which ... profound.” Nor sure what this sentence means? Impact of modes themselves is profound but it is uncertain how external forcing will alter them? Rewrite the sentence. U.S. Government
09	043	20		23	Why is there a prior expectation that modes will change? Also, what will change? U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	043	36		37	How does using a model relate to deducing what the observations are showing? Please explain. U.S. Government
09	043	26		39	Begin para with the sentence (line 37-39) “There is no detectable change in” U.S. Government
09	043	29			What is the relationship between more intense El Ninos and warming in the western tropical Pacific? Is this just coincidental or is there a casual relationship implied? U.S. Government
09	043	29			Why are intensified El Ninos related to warming of the western Pacific? Please clarify - is there causality here, or just a coincidence in time? U.S. Government
09	043	41	44	3	This discussion of PDO is not obviously needed in so far as it has no attribution statements as now written. Perhaps the point here should be that there are strong, outwardly natural, decadal fluctuations in the Npac, and that these influence an ability to detect climate change at this point in time. Consider if the result from one model represents a real attributable change; if not, either give this as a conclusion or consider having this whole section discussed in Chapter 3. U.S. Government
09	044	2		3	What does, if any, observational evidence suggests? Do the observations suggest a positive phase influence - or is the model result only for the future? U.S. Government
09	044	6			In chapter 8, pg. 30 line 1, there is a reference to the NAM as being 'not zonally symmetric', while here it says it is approximately zonally symmetric. The two chapters should be made consistent. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	044	19		36	For clarity, replace “increase in the NAM index” with “increase in the positive polarity of the NAM index” U.S. Government
09	044	46			High latitude SST changes, associated for example with sea ice reductions, also play an important role (hence both sea ice changes and NAO/AO changes feedback positively on one another-same Rind references). U.S. Government
09	044	49		54	It is not clear that the increasing Eurasian temp of recent decades have been “caused” by the NAM change, via air mass transport, or whether both have been driven by a third factor. Indications are that the NAM-sfcT relation over the past 100+ yrs has been nonstationary, and so the recent relation may be coincidental as far as cause-effect statements are concerned. See recent studies by Osborn (2004). The historical relationship provides feedback on the importance of NAO advection change, and should be considered. U.S. Government
09	045	18		22	No point in speculating on the mechanism for how the SP stratospheric AAO signal is communicated to the troposphere, as there are many competing theories. As now written, this Chapter appears to endorse the notion of altered propagation of planetary waves, by using the word “probably”. There is no evidence that I am aware of that indicates this to be any more probable than other mechanism, including simple dynamical adjustment consistent with PV inversion. Should mention several mechanisms and use the word 'possibly'. U.S. Government
09	045	42			What does this suggest? U.S. Government
09	045	47			This para is started with the essential attribution statement; suggest a similar style, where possible, be used throughout Chapter 9. U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	046	1			What gives rise to this expectation, given that rainfall intensities are expected to increase, so diabatic heating would as well? This is an important point, and if truly believed, the expectation should be explained further. On a broader point, if this is an attribution chapter, why is the future being discussed anyway? U.S. Government
09	046	5		39	This section on tropical cyclones needs a rewrite. This is arguably one of the more important attribution challenges, made so by recent events especially. A clear lead statement of what is new knowledge is needed. I could be the that the section begins with what now line 24. However, my own sense is that this section fails to highlight the weight of evidence that the most intense cyclones have increased in recent decades, that such an increase has been modeled with a CGCM using an embedded hurricane model (Knutson et al.), and that theory predicts such an outcome based on thermodynamic considerations. While one cant entirely disagree with the last sentence (lines 37-39), this is an objectionable sentence nonetheless in light of what new knowledge has accrued since TAR, and it understates the science's current attribution capabilities. U.S. Government
09	046	29			In Chapter 3 the Emanuel result comes with caveats that the data set is short and there is evidence of higher values earlier in the period. U.S. Government
09	046	41		57	The model and observation discussion of the same phenomena (storm intensity and frequency) should be placed together in the same paragraph. In addition, the attribution statement at the end should contain the 'likely', 'not likely' terminology for IPCC. U.S. Government
09	046	51			The SH storm tracks are associated with ENSOs and their movement (poleward or equatorward) varies in the different ocean basins with ENSOs. To the extent that there has been a trend toward more positive ENSOs, this will affect the storm track. U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	047	4			Suggest not beginning with a "predictive" statement.... "As climate warms, the amount of moisture in the atmosphere is expected to rise...". This is an attribution chapter, after all. U.S. Government
09	047	18			Remove the predicitive statement: "...can be expected to". Suggest stating what has happened, and then addressing the attribution.. U.S. Government
09	047	28		33	Suggest also looking at Kumar et al. 2004, Journal of Climate, 17, 653. Also don't start with predictive statement, but observed statement. U.S. Government
09	047	35			Taken literally, this discussion would imply that if short wave radiation isn't affected, there will be no change in global average precipitation regardless of what happens to greenhouse gases or atmospheric temperatures - is that what you really want to say? U.S. Government
09	049	48		55	Begin this section with the sentence "There have been suggestions of anthropogenic influences on precipitation on the regional scale, as discussed below." Delete the remainder of this para. U.S. Government
09	050	26		28	Is this quoting results from a single model? Add references to both the SSTs and, if available, land use part. U.S. Government
09	050	42		43	If this is the conclusion of the paragraph (rather than simply Hoerling's conclusion), then the emphasis given to the aerosol cause in the first part of the paragraph seems misleading. What is the assessment here? U.S. Government

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CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
09	052	40		46	Contradictory comments on these two lines: if winter accumulation positively correlated with temperature in the NH then why is there a decline in glacial mass balance with winter warming? Is it melting in winter (seems unlikely)? U.S. Government
09	053	40			What does this imply for future changes in precipitation? Are they really related only to short wave variations? U.S. Government
09	053	40	53	42	The statement implies that the observed sea surface temperatures were due to anthropogenic influence. Need to provide corresponding citations. U.S. Government
09	056	57			Is that what IPCC meant by the uncertainty range? U.S. Government
09	084				Should tone down the negativity in the first sentence, otherwise it raises the question of why this question is even being put forward? U.S. Government
09	085				Since this is supposed to be new since the TAR, why not phrase question 2: why has warming accelerated since the 1970s? U.S. Government
10	00				There are serious cross-chapter differences between 10 and 4 regarding the melting (4) or accumulation (10) of Antarctic ice with regard to sea level rise. The disconnect is serious, weakens the model projections here and should be resolved with careful language (realizing that it is a bit too late to resolve the model differences.) Please reconcile the projected negative contribution to sea level rise from Antarctica, with apparent current positive contribution discussed in Chapter 4. Are there two schools of thought that each need to be reflected in both chapters? U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
10	00				Please provide results for the low and high emissions scenarios. Justification: IPCC usually tries to characterize the range of uncertainty, but basing projections on a single emissions scenario may unduly narrow the range. Past efforts generally tried to ensure that each time different factors were considered to develop alternative temperature estimates (e.g. emissions, carbon cycle models), a corresponding estimate of sea level rise was also reported. U.S. Government
10	003	38		44	The use of ranges and the definition of what uncertainty means should be clearly stated and used consistently throughout the chapter. The two examples here are confusing. How you chose to calculate the range (+-20%) is not relevant here, but what is important is what likelihood that range covers (either 2/3, +-1 sigma, +- 2sigma). Please be explicit, you can define what ranges in temperature mean once and use it throughout. U.S. Government
10	003	48		53	Delete this paragraph. It is redundant with the findings presented in Chapter 9 and belongs in Chapter 9 alone. U.S. Government
10	004	45			Projections on sea level rise (without full uncertainty analysis) must also be present for the B1 and A2 scenarios as high and low projections. This is critical to avoid the appearance of selection. Either give the calculated numbers or make an expert judgment. Going without looks biased. Basing projections on a single emissions scenario may unduly narrow the range. Past efforts generally tried to ensure that each time different factors were considered to develop alternative temperature estimates (e.g. emissions, carbon cycle models), a corresponding estimate of sea level rise was also reported. U.S. Government
10	004	49		56	Please reconcile the projected negative contribution to sea level rise from Antarctica, with apparent current positive contribution discussed in Chapter 4. U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
10	010				Table 10.2.1: the caption does not incorporate all of the categories shown on the table and does not really explain the simulations. Fix the table. U.S. Government
10	015	5		7	The review of current/past stratospheric changes (sulfate, water vapor, etc) should not be done here as it may be in conflict with the more thorough and up to date coverage in Chapter 2 (where it belongs). Shorten this paragraph and just note the range of possible error in your future scenarios due to the lack of these forcings. Also see details on next comment. U.S. Government
10	015	08	15	11	Re: Marquart et al. (2003) estimate that the radiative forcing by contrails will increase from 0.035 W m ⁻² in 1992, to 0.094 W m ⁻² in 2015, and to 0.148 W m ⁻² in 2050. The rise in forcing is due to an increase in subsonic aircraft traffic following estimates of future fuel consumption (Penner et al., 1999). The projections cited for the year 2050 are based on static scenarios that only consider fuel burn from aviation changes. What would be the estimates of other ongoing dynamic changes (e.g., loading of GHG, circulation changes, etc.) are taken into account? U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
10	059	14	59	16	Consider the following comment regarding Chapter 4-10 cross-issues and see if a rewrite would help. Change “all studies for the 21st century find that Antarctic SMB changes contribute negatively to sea level, owing to increasing accumulation (see Chapter 4, Section 4.6.3 for comparison with changes in the last decade)” to “Although Chapter 4 reports that Antarctica has been contributing to sea level during the last decade, all studies for the 21st century find that Antarctic SMB changes contribute negatively to sea level, owing to increasing accumulation.” And then explain this apparent contradiction. Justification: The reference to Chapter 4 is so subtle that many readers would not realize that the Chapter 10 authors expect Antarctica to be much less sensitive to warming than the Chapter 4 authors suggest that it was in the past. U.S. Government
10	060	20			There is a serious problem here. The use of very unlikely is not justified (see below). Fix this with a change in global range or the certainty description. Also, please do not mix units (absolute C for one shelf, relative C for another; % for one range, factor for another). Change “uncertainties are not well characterized but these figures suggest that a local summer warming of 5°C is very unlikely for a global warming of less than <u>5°C</u> ” to “....global warming of less than <u>3-4°C</u> ”. Justification: The assertion is based on a previous sentence that says that summer warming will be 20-100% of the annual warming, which will be 70-150% of global warming. Combining those two factors implies that Antarctic summer warming will be 14-150% of global average warming. Given that range, without further analysis, it is reasonable to state that Antarctic warming is very unlikely to be more than 150% of global warming. However, this information provides no basis for asserting that it is very unlikely (or even unlikely) that summer warming will be more than 100% of global warming—100% seems to be well within the range of possibility. U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
10	060	20		23	The logic of this reasoning was difficult to follow (i.e., why the entire summer has to be above freezing for significant melting). If this is intuitive and can be explained as a modeling result, then try to help the reader here. Please consult Chapter 4 authors to determine whether Chapter 4 will be revised to provide some support. One obvious issue is whether the interannual and monthly variabilities are small enough so that high temperature periods (above the 0 C mean) are barely warmer than the seasonal average and hence do not lead to much melting. U.S. Government
10	060	47			Please explain whether sea level rise caused by other factors can also cause a dynamic retreat of the grounding line for the same reasons as thinning of the ice. This is a projections chapter and sea level is indeed projected to rise. U.S. Government
10	061	42		47	Please add a table with the models used for sea-level rise projections, and the primary contributors to the sea-level rise (Antarctica, Greenland,) for the year 2100 and possibly an intermediate year such as 2050. Coastal planners and engineers preparing for sea level and other users of IPCC sea-level rise projections have often needed to cite the contributions from different sources, which has been supported in past assessments. In addition, please report either the historic contribution that the models estimate, or report projections both as an absolute rise over a base year, and as an acceleration over current or historic trends. U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
10	061	42		49	<p>Results (without full uncertainty analysis) must also be present for the B1 and A2 scenarios as high and low projections. This is critical to avoid the appearance of selection. Either give the calculated numbers or make an expert judgment. Going without looks biased.</p> <p>Basing projections on a single emissions scenario may unduly narrow the range. Past efforts generally tried to ensure that each time different factors were considered to develop alternative temperature estimates (e.g. emissions, carbon cycle models), a corresponding estimate of sea level rise was also reported.</p> <p>U.S. Government</p>
10	061	49		56	<p>If not otherwise included in a table, please either (a) report the projected sea-level rise as an acceleration compared with current or historic trends, or (b) report the historic contributions to sea-level rise estimated by the models used to project future sea-level rise. Justification: Coastal planners and engineers, and other users of IPCC projections, have to make local sea-level rise projections. Although there are several methods for doing so, many people follow the approach recommended by USEPA, which is to add the current local trends (based on tide gages) to a model-derived estimate of the acceleration. That method can only use IPCC projections if IPCC reports enough information for one to determine their estimated acceleration. In that regard, it may be worth noting that some models project a rise in sea level of less than 15 cm—less than the rise over the last century. But those models do not project a deceleration; they simply have a baseline lower than historic trends. Nevertheless, if acceleration and/or the baseline for the model are not reported, some readers might be left with the impression that the low scenario is a deceleration of sea-level rise when in fact it is a small acceleration projected by a model that just happens to have a very low baseline. By analogy, if a GCM had a baseline that was 2°C colder than today and projected a 1°C warming, you would not leave readers with the impression that such a model implies a 1°C cooling. Leaving readers with the impression that the model implies a deceleration of sea-level rise provides a similarly incomplete picture.</p> <p>U.S. Government</p>

U.S. Government Comments on “Climate Change 2007: The Physical Science Basis”

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
10	072	10			This text box is very important and will stand out. It needs to be very carefully worded and consolidated with Chapter 9. Fro one, the term “climate sensitivity” has at least two meanings (equilibrium or transient 1%/yr) and is confused with confused with the climate sensitivity parameter. So please expand the title to be precise and unambiguous, perhaps add ‘uncertainty’ or ‘prob distrib’ to the box title. To justify its placement here, it needs to have a focus on providing a context for uncertainty estimates in the projections. U.S. Government
11	00				Overall figures in this chapter need a lot of improvement. The text on some of the plots is too small. U.S. Government
11	004	12			This is a policy statement and should be removed. Even adaptation belongs in WG3. U.S. Government
11	002		3		The entire Exec Summary is weak; it has a general review, but no real ES points. It needs some snappy, new real results: e.g., Does down-scaling work? What value is added by these techniques and this chapter that we cannot readily see from the global chapters? Are these results any different from analysis of 300 km global models? We realize that it is too late to re-organize the chapter, but the ES could be greatly shortened to what is new here. Given your own statements on p.2 line 8, what is new here? U.S. Government
11	005	17		18	The sentence “The difficulties ... apparent” is unneeded and should be dropped. The entire opening paragraph is also weak and needs to focus on “Developments since the TAR”. U.S. Government
11	005	51			Maybe you are trying to avoid overuse of the term “inter-grid distances,” which is used twice later in the paragraph, but this term is more precise and correct than “horizontal scales.” Have a consistent use; do not keep varying the jargon. U.S. Government

U.S. Government Comments on “Climate Change 2007: The Physical Science Basis”

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
11	006	49			Since the difference between the climate generated by different GCMs is not just due to natural (presume you mean ‘internal’ here) variability and predictability limits, but due to biases and differing reponses among the GCMs, replace “also” with “only” or “primarily.” Note that “ensemble” appear to be used incorrectly here as the average of different CGCMs rather than a set of runs from one. U.S. Government
11	011	1			Add two major categories to sources of regional uncertainty. The first is that shifts in circulation as depicted in GCMs can have considerable uncertainty and their effects may not be significant on a global scale, but they can have considerable impact on a regional scale. The second is the difficulty in understanding the relationship between grid spacing/spectral truncation of GCMs (and RCMs too) and the scale at which we should take their results as truly being representative (and the lack of awareness of this issue in the climate impacts community). U.S. Government
11	015	33			Do not forget to include [INSERT TABLE 11.2] call out. U.S. Government
11	018	34			Do you mean “poleward” here rather than “northward”? Please check exactly what shifts of the storm tracks you mean. U.S. Government
11	019	12			Rotstayn and Lohmann (2002) is not in the references list. U.S. Government
11	042	29			Should be Higgins and Mo 1997 rather than Higgins et al. 1997. U.S. Government
11	044	40			This should say “temperature change” rather than “climate change.” U.S. Government

CHAPTER	FROM PAGE	LINE	TO PAGE	LINE	COMMENT
11	045	35			You may wish to clarify that LLJ simulation is aided by "increases in both horizontal and vertical resolution." U.S. Government
11	049	34		55	Atlantic THC is used here to refer to the same thing that is called in SPM, page 15, Atlantic MOC. This should be consistent. Please check your usage here and throughout the chapter for consistency with the glossary and the other global climate chapters. U.S. Government
11	105	13	105	15	This citation is incomplete or misplaced. U.S. Government
11	114				Table needs to refer to Figure 11.2.1. U.S. Government
11	124				Figure 11.2.1 needs some work. In the title, 'vs' should be replaced by '-', to keep with standard convention of reporting differences between two periods. Legend on the left hand side should include 'Global'. Corresponding change of vs should also be made on page 11-12, line 48. Also page 11-12, line 48 did the authors mean average of temperature changes for December, January, February, or average of temperature change for DJF as in title for the figure. This needs to be clarified. U.S. Government
11	157				Box 11, Figure 2 may have some errors. For example, if I look at Balkan Europe in Fig 11.3.3.2 there seems to be a consensus of DJF ppt increase in that region, but Box 11 Fig 2 DJF shows the region as white, implying that 75% of the models show neither an increase nor decrease. This seems inconsistent. I have not tested this for other regions, but the authors should be encouraged to do so. In the JJA map the drought symbol hides some of the local detail; move the symbol into the Med. U.S. Government